

TRANSACTIONS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. XX. PART IV., FOR THE SESSION 1852-53.

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XXXV.—*Observations on the Speculations of Dr Brown and other recent Metaphysicians, regarding the Exercise of the Senses.* By Professor W. P. ALISON.

(Read 7th and 21st February 1853.)

IN offering to this Society a few remarks which have occurred to me on this fundamental department of Mental Physiology, I beg in the first place to explain, that my reason for doing so is merely this, that in consequence of certain unguarded expressions, and, as I think, hasty reflections, the opinions of Dr BROWN, and likewise of Sir JAMES MACKINTOSH, and of Lord JEFFREY, and other more recent writers on this subject, have been supposed to be irreconcilably at variance with those of Dr REID and Mr STEWART; *i. e.*, with those which are usually called the leading doctrines, or essential characteristics, of the Scotch School of Metaphysics, in this fundamental department of the science. And when such difference of opinion is believed to exist among men of generally acknowledged talent, who have studied this subject, and nothing like an *experimentum crucis* can be pointed out, to compel us to adopt one opinion and reject another,—the natural inference is, that there is something in the study itself, which renders it unfit for scientific inquiry,—that what is called the study of the Mental Faculties granted to our species is, in fact, only a record of the vacillations of human fancy and ingenuity, in the invention and demolition of hypotheses,—and that the subject is one on which it is in vain for our minds to dwell, with any hope of applying the principles of Inductive Science, and acquiring any insight into the laws of Nature, regulating the phenomena presented by the last and greatest of her works, similar to that which is the object and the reward of all other scientific inquiries.

When, for example, we find it stated by Dr BROWN, that on the first and most fundamental of all inquiries regarding the human mind,—that into the belief which attends the exercise of the Senses,—the creed of the sceptic, and of the orthodox philosopher of Dr REID's School, consists, in fact, of the same two propositions,—“and that what appeared to Dr REID and Mr STEWART to be the overthrow of a great system of scepticism on this subject, was nothing more than the proof that certain phrases are metaphorical, which were intended by their authors to be understood only as metaphors;”—(*Lectures*, vol. i., p. 584); when we find this statement of Dr BROWN's regarded by Sir JAMES MACKINTOSH as so just and important, that he says, “the whole intellectual part of the philosophy of BROWN is an open revolt against the authority of REID;—Mr STEWART had dissented from the language of REID, and departed from his opinions, on several secondary theories; Dr BROWN rejected them entirely,—*very justly* considering the claim of REID to the merit of detecting the universal delusion, which had betrayed philosophers into the belief that Ideas, which were the sole object of knowledge, had a separate existence, as

a proof of his having mistaken their illustrative language for a metaphysical opinion;"—and when we remember the unparalleled popularity of Dr BROWN's Lectures since his death, which has no doubt led many to suppose that he is now regarded as the first authority on these subjects in Scotland;—when we find, again, Lord JEFFREY admitting that "Dr REID's subversion of the ideal system, or confutation of that hypothesis which represents the immediate objects of the mind in perception as certain images or pictures of external objects conveyed by the senses to the sensorium, had been performed with complete success;" but adding "that after considering the subject with some attention, he has not been able to perceive how the destruction of the Ideal Theory can be held as a confutation of those reasonings, which have brought into question the popular faith on this subject;" (*Edinburgh Review*, vol. iii., p. 281; or *Contributions to Edinburgh Review*, vol. ii., p. 604)—when, on the other hand, we find it stated by Sir WILLIAM HAMILTON, that Dr BROWN is, from first to last, in one and all of his strictures on REID's doctrine of Perception, wholly in error;" but, "that nevertheless there are ambiguities and inconsistencies of REID himself, in this the most important part of his philosophy, which ought to be exposed, and so deprived of their evil influence;" (*Works of Reid*, p. 820)—"and, in particular, that so far from confuting Idealism, the doctrine of REID and STEWART affords it the best of all possible foundations;" (*Works of Reid*, p. 820)—and, again, by MORELL, that although REID "performed an inestimable service to philosophy, by shewing that certain simple processes must be viewed as ultimate and primitive facts in our constitution,—the benefit of which is still to be developed in coming generations,—yet that the false, or at least inadequate view which he has taken of the *reflective method* in mental philosophy, has caused a want of comprehensiveness as to the legitimate objects or extent of philosophy at large;" (*Morell's Philosophy*, vol. ii., p. 51)—I think I have quoted enough to shew, that a general distrust of all speculations which led such men to such variety of opinion, and despondency as to the possibility of any fixed or useful principles being established, by scientific examination of the elementary mental processes to which they referred, is not unreasonable. And if, nevertheless, we hold, as I think we ought to do, by the maxim, "that when Reason and Philosophy have erred, it is by themselves alone that their error can be corrected,"—I trust it will not be deemed a useless or unprofitable discussion, to endeavour to shew that when the subject is calmly reviewed, and verbal ambiguities as far as possible avoided, there is really no such difference of opinion among these authors, as will justify the strong expressions of dissent which I have quoted; but that the differences of opinion are either verbal only, or relate to matters *ulterior to the main points of controversy*, which have interested the human mind, in all ages, on this subject; and, in particular, ulterior to those on which it was the object of REID and STEWART to establish fixed and satisfactory principles; and that there are certain general truths

in regard to the mental part of the process concerned in the exercise of the senses,—probably admitting of much more subtle analysis, and more learned discussion than I can presume to offer,—but already sufficiently certain and precise, to constitute an important part of the science of Physiology; and remarkably in accordance with all that has since been ascertained, as to the physical part of that process.

As I was myself honoured in early life with the friendship both of Mr STEWART and Dr BROWN, and as I know well how much the former of these illustrious men was pained by finding that the latter, when succeeding him in the Chair of Moral Philosophy, had (as he afterwards expressed it) “given countenance to some doctrines, which, to more cautious and profound thinkers, appear to have a practical tendency quite at variance with his known principles and opinions;” (*Elements of the Philosophy of the Mind*, p. 502)—although I believe that the natural partiality of Mr STEWART to the studies to which he had devoted his life, had led him to exaggerate, in some degree, their practical importance,—still I feel much gratified at being able, as I think, in some measure to reconcile the apparently conflicting statements in their writings, and point out the misapprehensions—almost entirely on the part of Dr BROWN—to which they may be traced.

It will be generally admitted, that the first object of REID and STEWART was to ascertain, by strict induction, the existence, and establish the authority, of certain *Principles of Common Sense*, as they were termed by REID; *Primary Elements of Human Reason*, or *Fundamental Laws of Human Belief*, as they were termed by STEWART; *Principles of Intuitive Belief*, or *Truths learned by Intuition*,—perhaps the best name for them,—as they were since termed by BROWN; which must be regarded as *ultimate facts* in the constitution of the human Mind, equally essential to all reasoning, to all scientific inquiry, to the acquisition of all practical knowledge, and to the daily business of life.

Now the existence of such principles of Belief, and their authority, as ultimate facts in our mental constitution, are fully admitted by all the authors I have quoted; by no one are they more clearly and emphatically announced than by Dr BROWN. “Principles of intuitive belief,” he says, “are essential to Philosophy in all its forms, as they are physically essential, indeed, to the very preservation of our animal existence.” “The belief of *our identity* is not the result of any series of propositions; but arises immediately, in certain circumstances, from a *Principle of thought*, as essential to the very nature of the Mind, as its powers of Perception or Memory, or as the power of Reasoning itself; on the essential validity of which, and consequently on the intuitive belief of some *first truth* on which it is founded, every objection to the force of these very truths themselves must ultimately rest. To *object* is to *argue*; and to *argue* is to assert the *validity* of *argument*, and therefore of the primary evidence, from which the evidence of each succeeding proposition of the argument flows. To object to the authority of such primary

intuitive belief, would thus be to reason against reason,—to affirm and deny at the same moment,—and to own that the very arguments which we urge are unworthy of being received and credited.

“ Without *some* principles of immediate belief, then, it is manifest that we could have no belief whatever; for we believe *one* proposition because we discover its relation to *some other proposition*; and we must ultimately come to some *primary* proposition, which we admit from the *evidence contained in itself*, or, to speak more accurately, which *we believe from the mere impossibility of disbelieving it*. All reasoning, then—the most sceptical, be it remarked, as well as the most dogmatical—must proceed on *some* principles which are taken for granted, not because we infer them by logical deduction, but because the admission of these first principles is a necessary part of our intellectual constitution.

“ Every action of our lives is an exemplification of some one or other of these truths, as practically felt by us. Why do we believe that what we *remember* truly took place, and that the course of Nature will be in future such as we have already observed it? Without the belief of these physical truths, we could not exist a day, and yet there is no reasoning from which they can be inferred.

“ These *principles of intuitive belief*, so necessary for our very existence, and too important, therefore, to be left to the casual discovery of Reason, are, as it were, an eternal, never-ceasing voice from the Creator and Preserver of our being. The reasonings of men, admitted by some and denied by others, have over us but a feeble power, which resembles the general frailty of man himself. These internal revelations from on high are omnipotent, like their Author. It is *impossible* for us to doubt them, because to disbelieve them would be to deny what our very constitution was formed to admit.”—(BROWN, p. 286.)

The principle thus stated by Dr BROWN, and some of the illustrations of it which he has given, seem to me to be worthy of all acceptance; but I beg to ask, how do they differ from the fundamental proposition of Dr REID'S Philosophy of Common Sense; long previously set forth, for example, in the following passage? If there is no essential difference, then I think it clear that Dr BROWN ought to have distinctly intimated his acquiescence in this, which Dr REID regarded as the cardinal point of his doctrine; and so far, by limiting and defining the province of reasoning, and that of simple observation in such inquiries, endeavoured to prevent useless labour, and irksome uncertainty, in future students of the same science.

“ All reasoning must be from First Principles; and for first principles no other reason can be given but this, that, by the constitution of our Nature, we are under a necessity of assenting to them. Such principles are parts of our constitution, no less than the power of thinking; Reason can neither make nor destroy them, nor can it do anything without them.

“ How, or when, I got such first principles, upon which I build all my rea-

soning, I know not, for I had them before I can remember; but I am sure they are parts of my constitution, and that I cannot throw them off. That our thoughts and sensations must have a subject, which we call *ourself*, is not an opinion got by reasoning, but a natural principle. That our sensations of touch indicate something external, extended, figured, hard or soft, is not a deduction of reason, but a natural principle. The belief of it, and the very conception of it, are equally parts of our constitution. If we are deceived in it, we are deceived by Him that made us, and there is no remedy."—(*Works of REID*, by Sir W. HAMILTON, p. 130.)

"I beg," he says farther, "to have the honour of making an addition to the sceptical system, without which I conceive it cannot hang together. I affirm that the belief of the existence of Impressions and Ideas, is as little supported by reason, as that of the existence of Minds and Bodies. No man ever did, or ever could, offer any reason for this belief. A thorough and consistent sceptic will never therefore yield this point; and while he holds it, you can never oblige him to yield anything else.

"To such a sceptic I have nothing to say; but of the semi-sceptics, I should beg to know, why they believe the existence of their own impressions and ideas? The true reason I take to be, because they cannot help it; and the same reason will lead them to believe many other things."—(*Do.*, p. 130.)

In quoting this last passage from Dr REID, I think it right to say, that notwithstanding his distinct assertion here made, and supported by Mr STEWART, that the evidence of *Consciousness* (by which we are informed of the acts of our own minds) stands on exactly the same footing as that of *Sense*, and is equally open to the objections of the sceptic, it seems to me that the objection to that statement, made by several more recent authors, is well founded; because what we mean by objects of consciousness are certain changes or events which we feel within ourselves, and we cannot, *without absurdity*, assert, both that such a change exists, *i. e.*, that we feel it, and that we doubt its existence, which implies that it may not exist. To doubt the evidence of consciousness, therefore, is not merely to do violence to our understandings, but is to assert a contradiction in terms.

This is thus stated by Lord JEFFREY: "Whatever we doubt, and whatever we prove, we must plainly begin with Consciousness. *That only is certain—all the rest is inference.* Our perceptions—not the existence of their objects—are what we cannot help believing."—(*Review*, vol. iii., p. 283.) And the same ground is taken by Sir WM. HAMILTON thus: "There is no scepticism possible touching the facts of consciousness *in themselves*. We cannot doubt that the phenomena of consciousness are real, in so far as we are conscious of them, because such doubt, being an act of consciousness, would contradict, and consequently annihilate itself: but all beyond the mere phenomena of which we are conscious, we may, without fear of self-contradiction at least, doubt."—(*Works of REID*, &c., p. 129.)

But granting that this criticism is correct, the only alteration we need make on the passage last quoted from REID is this, that, instead of asking the "semi-sceptics why they believe in the existence of their impressions and ideas," we should ask them, why they believe that the impressions and ideas of which they are *conscious*, are *their own*, or belong to the same persons as other mental changes which they *remember*. Here we become involved with the evidence of Memory and of Personal Identity, as to both of which Dr BROWN expressly admits, in passages already quoted, that they are to be ranked among the principles of Intuitive Belief; and with that slight correction, this passage from Dr REID—closely approximating, as it obviously does, to that previously quoted from Dr BROWN,—must have commanded his entire acquiescence.

The reality and importance of these principles, regarded as ultimate facts in our mental constitution, is still more satisfactorily attested by Sir WM. HAMILTON, who has marshalled an array of authorities, such as any other man in this country might have in vain attempted, amounting to more than a hundred ancient and modern writers, all of whom, under certain varieties of expression, have announced and illustrated the same general proposition.

Farther, not only have we this nearly uniform agreement of these philosophers in regard to the general statement of REID, that "there are various acts of our minds, of which, when we analyse them as far as we are able, we find Belief to be an essential ingredient;" but we have a special agreement, of all those whose opinion is thought of much weight, as to the fact of the exercise of the Senses being one of the occasions, in which evidence of this description, whether directly or indirectly, is at least uniformly and essentially concerned.

The shortest and simplest account of Dr REID's doctrine on this subject is given by him in the following words: "The external senses have a double province, to make us *feel*, and to make us *perceive*. They furnish us with a variety of *sensations*, pleasant, painful, or indifferent; at the same time, they give us a *conception*, and an *invincible belief of the existence* of external objects. This conception of external objects is the work of Nature; the belief of their existence is the work of Nature; so also is the sensation that accompanies it. The *conception and belief*, which Nature produces by means of the senses, we call Perception."

He thus introduces the Intuitive Belief, simply as a part or accompaniment of the operation of the mind which results, in the healthy state, from an impression made on the senses, and a Sensation excited in the mind; and afterwards he enters on explanations in regard to the different qualities attributed to the material objects thus made known to us,—particularly as to the distinction of the Primary and Secondary qualities of matter, and the formation of the general notion or conception of Extension or Space; which, as he says, is no sooner formed, than it swells in the human mind to Infinity, as surely as the notion of Time to Eternity; and affords, therefore, the simplest illustration of the essential distinction between

Sensations felt in the mind, and Perceptions formed in the mind, in consequence of those sensations.

But although he stated that he could trace the formation of our notions in regard to the external world no farther than the mental operations thus described, he distinctly admitted, as a general principle, the possibility, and approved the attempt, of farther analysis, if made under due precautions. "It must," he says, "require great caution, and great application of mind, for a man that has grown up in all the prejudices of education, fashion, and philosophy, to unravel his notions and opinions, till he find out the simple and original principles of his constitution, of which no account can be given but the will of our Maker.

"This may be truly called an *analysis* of the human faculties; and till this is performed, it is in vain we expect any just system of the mind,—that is, an enumeration of the original powers and laws of our constitution, and an explication from them of the various phenomena of human nature. Success, in an inquiry of this kind, it is not in human power to command; but perhaps it is possible, by caution and humility, to avoid error and delusion. The labyrinth may be too intricate, and the thread too fine, to be traced through all its windings; but if we stop where we can trace it no farther, and secure the ground we have gained, *there is no harm done; a quicker eye may in time trace it farther.*"—(REID, p. 99.)

It is plain, therefore, that it was quite in accordance with Dr REID's views,—both with the principles which he thought he had established, and with his anticipations of the future progress of the science,—to attempt a farther and more minute analysis of the acts of Mind, attending the exercise of the Senses, by which we are assured of the existence; and informed of the properties of external things; and to endeavour to refer these, by a process of induction, to other and more general Laws of Mind. This, accordingly, has been attempted by several later writers. Mr STEWART maintained, and fortified himself by the opinion of TURGOT, that in order to the formation of the notion of Externality, or independent existence, in any object of perception, a repetition of the same sensations, under the same conditions, is necessary, and that then the formation of that notion,—the conclusion thus drawn as to the existence of a cause for our sensations, independent of ourselves, might be referred to the general Law of Mind,—analogous to the first Law of Motion, or the Inertia of Matter,—our belief "that the course of Nature is uniform, or will be in future such as we have already observed it."

Dr BROWN went a step farther. He explicitly admitted the accuracy of the distinction drawn by Dr REID between Sensations and Perceptions, and the convenience of the term Perception, as denoting an act of the mind, distinguishable from all others, but, as he thought, resolvable into others. "I am far from wishing," he says, "to erase the term Perception from our metaphysical vocabulary. On the contrary, I conceive it to be a very convenient one, if the meaning attached to it be sufficiently explained, by an analysis of the complex state of mind which

it denotes."—(*Lectures*, vol. ii., p. 47.) And he made a very ingenious attempt (such as Dr REID, from his expressions above quoted, I think, must have approved) to explain how the notion of the primary qualities of matter may be gradually formed, by the help of experience, in the mind.

"Perception," he says, "is only another name for certain associations and *inferences* which flow from other more general principles of the mind."—(Vol. i., p. 569.) He then goes on to explain how, by means of certain sensations, and particularly of those *muscular sensations*, consequent on the excitement of instinctive and voluntary muscular actions, which he has so ingeniously illustrated, the notion of the qualities of matter may be gradually introduced into the human mind. He distinguishes the Primary Qualities of Matter, I think, more satisfactorily than REID, or perhaps any other author has done, as the different modifications of Extension and Resistance; "the very notion of which combined," he says, "seems necessarily to indicate a material cause, or rather, is truly that which constitutes our very notion of Matter."—(Vol. i., p. 574.)

I am much inclined to think, although I would not state it as certain, that his very ingenious analysis of the mental acts suggesting this notion, *as it is often suggested,—i. e., regarding it as the natural result of muscular sensations, repeatedly excited, and again obstructed, in different degrees, at different points, and for different periods of time,—*is correct; if so, it affords as good an example as can be given, of what his friend Mr CAMPBELL called "the mysterious, and almost miraculous subtilty of his mind." But I maintain with confidence, that it does not in the slightest degree invalidate the statement of REID, as to the Belief which accompanies this act of the mind being a case of that Intuitive Perception of Truth, which we have seen that BROWN, equally as REID, admitted as the foundation of all knowledge and all reasoning; and that for two reasons:—

First, Dr BROWN expressly admits, that the perception of the primary qualities of matter *may take place without any such process of repeated muscular contraction and reasoning thereupon*; and that it does so in the lower animals, in whom the very first complex act of perception may often be observed to be *instantaneous*, and yet perfect, and its suggestions correct. "The calf and the lamb," he says, "newly dropt into the world, seem to measure forms and distances with their eyes almost as distinctly as the human reason measures them after all the acquisitions of his long and helpless infancy."—(Vol. ii., p. 70.)

The well-known observation of the chicken and the spider shews that, in other classes of the lower animals, this primitive instinct, or suggestion, as he calls it, is still more obvious. It is therefore, as he states it, only a question of observation and experiment, whether or not, in man as in other animals, Nature does communicate information by *intuitive* suggestion consequent on sensation,—which is neither contained in, nor logically deducible from, the sensation, but is, nevertheless, correct.

But *secondly*, the analysis which he offers of this act of mind, as usually performed by man, only professes to resolve the act, which REID called Perception, and regarded as an ultimate fact, into other principles or laws of thought, which Dr BROWN himself regards as ultimate facts; particularly into the principle that "we must suppose a cause for all our feelings" (vol. i., p. 565); the "*intuitive belief*" that what has been as an *antecedent*, will be followed by what *has* been as a *consequent*" (do., p. 514); the notion of *Time*; the belief in the Suggestions of Memory (do., p. 553); and the principle of Association or Suggestion (do., p. 565). "I do not conceive," says he, "that it is by any *peculiar* Intuition we are led to believe in the existence of things without. I consider this belief as the effect of that more *general Intuition* by which we consider a new consequent, in any series of accustomed events, as a sign of a new antecedent, and of that equally general principle of association, by which feelings that have frequently co-existed flow together, and constitute afterwards one complex whole."—(Vol. i., p. 518.)

The fact that notions are formed in the Mind of the properties of Matter, perfectly distinct from the sensations which excited them, and to be explained only by reference (sooner or later) to what we call *Intuition*, remains, therefore, as REID stated it; and is indeed strongly illustrated and confirmed by the elaborate analysis of the mode of their formation, attempted by Dr BROWN.

On the other hand, a fundamental part of the doctrine of KANT, as I understand it, and to which Sir WILLIAM HAMILTON is disposed to assent, is, that the notion of *Extension or Space*, which Mr STEWART thought it important to distinguish from the other primary qualities, as what he called one of the Mathematical affections of Matter, ought to be regarded as a necessary condition, or native element or *form of thought*; and that a belief in the existence of "an extended world, external to the mind and even to the organism, is not a faith blindly created, or instinctively determined, on occasion of a sensation; but exists in, or as a constituent of, Perception proper, as an act of Intuition or immediate knowledge."—(*Collected Works of REID*, p. 883.)

Whether this is really an improvement on the doctrine which he states, in connection with it, as that of Dr REID [*viz.*, "that on occasion of a Sensation, along with a notion or conception, constituting the Perception proper, there is blindly created in us, or instinctively determined, an invincible belief in its existence"], or whether this distinction is really verbal, I do not presume to decide; but I think it must be admitted, that this opinion is truly an *addition* to the statement of REID, and does not *stand opposed* to it; inasmuch as REID says only, "that the conception and belief are the work of Nature;" and this, of course, does not exclude the evidence that may be adduced in favour of any particular mode, in which we may suppose that Nature accomplishes the work; as, indeed, we have already seen that both STEWART and BROWN supposed it to be performed

by help of the general law of belief in the continuance of the order of Nature, which it had not occurred to REID to connect with it.

But if there be, as I maintain, this perfect *accordance* between the principles of Dr REID and the elaborate attempt of Dr BROWN, as of other later authors, to analyse those operations of mind to which the term Perception has been restricted by both, we may be pretty well assured that any *difference* of opinion among those authors, on this subject, can be of no great scientific importance; and may very probably resolve itself into one of those partial controversies, involving more or less of personal jealousy, which, we must admit, have disfigured and retarded most sciences.

We may next ask, then, how it should happen that Dr BROWN should have thought himself justified in dwelling at great length on what he called an *extraordinary mistake* made both by REID and his followers, as to the evidence of Sense, and the scepticism of BERKELEY and HUME regarding it?—how he should have been led to infer, and been at such pains to prove, that there is no real difference between the creed of the sceptic and that of the orthodox philosopher of Dr REID's school as to the evidence of sense; and how Sir JAMES MACKINTOSH should have been led to assert the whole intellectual part of the philosophy of BROWN to be, by reason of their *difference* on this very subject, an open revolt against the authority of REID?

The reason of this is, that both these authors, and other recent writers, as it appears to me, certainly misconceived and misrepresented the controversy as it was carried on during last century, in several particulars. I do not say that there may not have been partial mistakes on the part of Dr REID, particularly as to the exact meaning of previous authors,—and certainly there is in his writings a diffuseness of style, and frequent repetition of statements which might have been more impressive if more condensed;—but the chief misapprehensions affecting the principles which I have stated, were clearly on the side of BROWN.

I. It was a palpable misconception on the part both of Dr BROWN and Lord JEFFREY, to attribute to Dr REID the attempt to prove, by reasoning, the existence of the material world, in opposition to the scepticism of previous authors.

Thus Dr BROWN speaks of REID's "*supposed proof of the existence of a material world*," as quite inadmissible (vol. ii., pp. 50, 51); and Lord JEFFREY speaks of his destruction of the Ideal Theory as "*having been held as a demonstration of the real existence of matter*."—(*Edinburgh Review*, vol. iii., p. 281.) Whereas they ought to have observed that REID had, in a few simple but weighty words, disclaimed, as expressly as it is possible to conceive, any intention of attempting, or belief in the possibility of obtaining, such proof. He says, "Many eminent philosophers have laboured to furnish us with *reasons for believing our senses*; but their reasons are *very insufficient, and will not bear examination*."—(*Collected Works*, p. 328.) "Man's knowledge of what really exists, or ever did exist, comes by a channel

which is open to those who cannot reason. He is led to it in the dark, and knows not how he comes by it." "The pride of philosophy has led some to invent vain theories to account for this knowledge; and others, *who see this to be impracticable*, to spurn at a knowledge which they cannot account for, and vainly endeavour to throw it off. But the wise and humble will take it as the gift of Heaven, and endeavour to make the best use of it."—(*Ibid*, p. 330.)

Consistently with this statement, it is plain that Dr REID's object (as expressly avowed by Mr STEWART, *Phil. Essays*, p. 551, published in 1810, prior to Dr BROWN's first course of Lectures on this subject), in this department of the science, could not be to prove by argument the existence of the material world, but only to *refute the argument against it*; and to put our belief in it on the footing of one of those Intuitive principles, the existence of which we have seen that Dr BROWN fully admitted and illustrated, as being essential to all knowledge and all reasoning, and tacitly admitted in all inquiries and all arguments; therefore, to put scepticism on this subject on the same footing as that of the "thorough and consistent sceptic, who will not believe in the suggestions of his own memory, or the identity of his own person," to whom Dr REID had explicitly avowed, that "he had nothing to say;" and whose scepticism, as we have seen, Dr BROWN regarded in precisely the same light.

II. It was quite a misconception to suppose that the creed of the sceptics of those days was merely, as Dr BROWN states it, the *negative proposition* that the independent existence of the material world cannot be proved by reasoning,—or, as he expresses it, "that no argument can be offered to shew, by mere reasoning, the existence of external causes for our feelings."—(*Sketch of a System, &c.*, p. 143.) If this had been their principle, the words above quoted prove, that it must have commanded the entire acquiescence of Dr REID. But their creed,—so plausibly supported, and so ingeniously deduced from the language of the most esteemed metaphysicians then generally known, as to have a practical bearing which we can hardly realise in this generation,—was the *positive proposition*, that Reasoning compels or necessitates our *disbelieving* that independent existence, as involving an absurdity.

The opinion of the ablest judges, says Dr REID (in his first work, published in 1764), when speaking of the reasoning of BERKELEY as to "the evidence of the senses, seems to be, that these arguments neither have been nor can be confuted, and that *he has proved by unanswerable arguments, what no man in his senses can believe.*"—(*Collected Works*, p. 101.)

The object of HUME, says Mr STEWART, obviously was, "to inculcate a universal scepticism; not, as some have supposed, to exalt reasoning, in preference to our instinctive principles of belief, but, by illustrating the *contradictory conclusions* to which our different faculties lead, to involve the whole subject in the

same suspicious darkness;—not to *interrogate* Nature, with a view to the discovery of truth, but, by a *cross-examination* of Nature, to involve her in such contradictions as might set aside the whole of her evidence, as good for nothing." (*Phil. Essays*, p. 56.)

The argument of BERKELEY and HUME, although expressed in various terms, seems in substance to have been always this,—That we are *made* acquainted with any existence external to ourselves only by means of our own Sensations, *i. e.*, of certain acts or states of our own minds; or, as they usually expressed it, by *ideas in our own minds*; that any such external objects as exist must be the *exact images or prototypes of these ideas or mental states*, and that it is *absurd to assert* that an act or state of mind, whether called sensation or idea, can be the exact image or resemblance of any thing but another act of the same, or some other mind.

The following passage from Mr HUME is given by Dr REID, as the shortest and clearest exposition of the argument which he had anywhere found:—

"The universal and primary opinion of all men, that we perceive external objects, is soon destroyed by the slightest Philosophy, which teaches us, that nothing can be present to the mind but an image or Perception;" (the distinction of which term from Sensation, was not recognised by HUME), "no man who reflects, ever doubted that the existences which we consider when we say *this house*, and *that tree*, are nothing but perceptions in the mind, and *fleeting copies and representations of other existences which remain uniform and independent*. So far, then, we are necessitated by reasoning to depart from the primary instincts of nature, and to embrace a new system with regard to the evidence of our senses." To the same purpose we have the explicit declaration of BERKELEY, "that the existence of bodies, out of a mind perceiving them, is not only impossible, but a *contradiction of terms*."

This is not, as Dr BROWN stated it, "a mere negative assertion, that the existence of external things cannot be proved by argument" (vol. ii., p. 55), but as Dr REID had said, a distinct *positive assertion*, that argument or reasoning does *compel*, or *necessitate*, our departing from the belief in that existence, as involving an absurdity or contradiction. It was these positive but puzzling, and even humiliating assertions, and these only, that Dr REID undertook to confute.

III. It was quite a misconception to assert, as Dr BROWN repeatedly and confidently did, that the term *Ideas*, in the language of HUME, or of any philosopher after LOCKE, was to be understood *only* metaphorically or figuratively, as an expression for acts or states of mind, and did not imply belief in the existence of anything intermediate between the mind and the external objects of sense.

He shewed, indeed, that the term had been used *occasionally* in that metaphorical sense by various authors; which Dr REID knew, and regarded as a proof of its

being ambiguous, and therefore inconvenient. But we have already seen, that Mr HUME expressly asserted that the existences which we consider when we speak of objects of sense, are "*fleeting copies and representations of other existences which remain uniform and independent*;" and his notion as to the nature of these fleeting copies is farther shewn in another passage, as follows,—“No external object can make itself known to the mind *without the intervention of an image*, and of these images the most obvious of the qualities is *extension*.”—(*Treatise on Human Nature*, vol. ii., p. 416.) Has not Mr LOCKE expressly told us, says Mr STEWART, “that the *ideas of primary qualities of matter are resemblances of them*; and that *their patterns do really exist in the bodies themselves*,” and did not Mr HUME understand this doctrine in the most strict and literal meaning of words when he stated, “as one of its necessary consequences, that the mind either is no substance, or is an extended and divisible substance, *because the idea of extension cannot be in a substance which is indivisible and unextended*?”—(*Phil. Essays*, p. 553.)

This is surely enough to shew that what LOCKE and HUME called Ideas, had, according to them, a physical (not merely metaphorical) existence, and were essentially distinct from the mere acts or states of the mind itself. And as to BERKELEY, we have the distinct admission of Dr BROWN himself, that he evidently considered ideas “not as states of the individual mind, but as separate things existing in it, and capable of existing in other minds, but in them alone.”—(*Lect.* vol. i., p. 523.) On which he very justly afterwards observes, that “a mind containing, or capable of containing, *something foreign* within itself, and not only one foreign substance, but a multitude of foreign substances at the same minute, is no longer that simple indivisible existence which we term spirit.”—(*Lect.*, vol. i., p. 525.) But these statements are obviously and irreconcilably inconsistent with Dr BROWN’s subsequent assertion, that the word Idea was used by *all* previous authors only metaphorically, and that in proving ideas not to be self-existent things, REID had merely assumed as real what was intended as metaphorical.

It is still more remarkable, that the notion which was taken up by Dr BROWN, of the language of HUME and BERKELEY having been only metaphorical or figurative, is the very same as had been previously hazarded by PRIESTLEY, and previously answered, and shewn to be inconsistent, both with the language of these and other philosophers, and with his own language, by Mr STEWART in his *Philosophical Essays*.

“The following strictures,” says Mr STEWART, “on REID’s reasonings against the Ideal Theory, occur in a work published by Dr PRIESTLEY in 1774:—

“Before our author had rested so much upon this argument, it behoved him, I think, to have examined the strength of it a little more carefully than he seems to have done; for he appears to me to have suffered himself to be misled in the very foundation of it, merely by philosophers happening to call Ideas the *images* of external things; as if this was not known to be a *figurative expression*, denoting,

not that the actual shapes of things are delineated in the brain, or upon the mind, but only that impressions of some kind or other are conveyed to the mind by means of the organs of sense, and their corresponding nerves, and that between those impressions and the sensations existing in the mind, there is a real and necessary, though at present an unknown connection."

On this passage, Mr STEWART observes, "To those who have perused the metaphysical writings of BERKELEY and of HUME, the foregoing passage cannot fail to appear much too ludicrous to deserve a serious answer. Where did he learn that the philosophers who have happened to call ideas the images of external things, employed this term as a figurative expression?"

He then contrasts it with some of the expressions of LOCKE and of HUME, which I have already quoted, and afterwards proceeds to shew, that it is utterly inconsistent with the following passage in a subsequent work of Dr PRIESTLEY himself,—“Whatever ideas are in themselves, they are evidently produced by external objects, and must therefore correspond to them; and since many of the objects or archetypes of ideas are divisible, it necessarily follows, that the ideas themselves are divisible also. The idea of a *man*, for instance, could in no sense correspond to a man, which is the archetype of it, and therefore could not be the idea of a man, if it did not consist of the ideas of his head, arms, trunk, legs, &c. It therefore consists of parts, and is consequently divisible. And how is it possible that a thing (be the nature of it what it may) that is divisible, should be contained in a substance, be the nature of it likewise what it may, that is indivisible.” If the “archetype of ideas have extension, the ideas expressive of them must have extension likewise; and therefore the mind in which they exist, whether it be material or immaterial, must have extension also.”

“No form of words,” says Mr STEWART, “could have conveyed a more unqualified sanction than he has here given to the old hypothesis concerning Ideas, —a hypothesis which he had before asserted to have been never considered by any philosopher but as a figurative mode of expression; and which, when viewed in the light of a theory, he had represented as an absurdity too palpable to deserve a serious refutation.”—(*Phil. Essays*, p. 554.)

Mr STEWART afterwards refers, in the same work, to the passages which I shall presently quote from Dr REID, as containing the true statement of his reply to the sceptical argument of BERKELEY and HUME; founded, as he believed it to be, on the language of LOCKE, and of what have since been termed the Sensational School of Metaphysicians; and farther refers to several prior authors, particularly BAXTER in this country, and D’ALEMBERT in France, as having stated and pointed out the importance of the same principle that REID did, but without illustrating it sufficiently.—(See *Phil. Essays*, Notes and Illustrations, p. 55.)

I cannot conceive that Dr BROWN should have made the statements which I have quoted, and which Sir JAMES MACKINTOSH and others have approved, as to

the language of HUME and others having been merely metaphorical,—and should have pronounced, on that ground, the claim of Dr REID to a refutation of their scepticism to have been inadmissible, without making the least reference to Mr STEWART'S answer to the very same objection when made by PRIESTLEY, and without mentioning the passages in REID and other authors to which Mr STEWART had referred, as the true exposition of this argument,—if he had read or reflected on those passages in Mr STEWART'S writings; and yet they were published in his *Philosophical Essays* in the summer of 1810, *i. e.*, some months before the first course of lectures which Dr BROWN delivered as Professor of Moral Philosophy in Edinburgh. But those who are aware of the peculiar sensitiveness of Dr BROWN'S physical constitution, of the painful effort which he made to prepare his lectures for that first course, and of his unwillingness at any subsequent time to revert to that part of his subject, on which indeed his lectures subsequently underwent only verbal alterations, will feel no difficulty in understanding, that one of Mr STEWART'S essays (the second in the volume of *Philosophical Essays* published in 1810), and the notes to it, may either not have been read, or read so hastily as to have been speedily forgotten by Dr BROWN, and never recurred to his mind when he was either revising his lectures, or preparing the short abstract of this portion of them which was published only a few months before his death.

It is only doing justice to the candour and discernment of the late Dr WELSH to observe, that in stating, in his life of Dr BROWN, the argument drawn from what he considered to be only the metaphorical use of the term *Idea*, in opposition to Dr REID'S argument, he took notice of what he termed “the defence of REID'S views, contained, as if by anticipation, in Mr STEWART'S *Philosophical Essays*,”—*i. e.*, contained in a work published before Dr BROWN'S lectures containing that argument were delivered, if not before they were written. It was perhaps unfortunate that Dr WELSH merely referred to Mr STEWART'S argument, and to some of the extracts from former authors by which it was supported, without quoting them, or expressing any opinion of his own on the subject. (See *Life of Dr BROWN*, p. 259.) And it is still more unfortunate that Mr STEWART himself, in the essay in question, and the notes to it, although he refers to the passages in REID'S writings, which I shall presently quote, as containing the true statement of his argument, did not quote any of his words.

IV. But farther, keeping always in mind that Dr REID'S avowed object was, not to prove by reasoning the existence of the material world (which he expressly avowed to be impossible), but only to confute the argument which represented that belief as an absurdity, I would observe that it was quite a misconception to suppose, as both Dr BROWN and Lord JEFFREY did, that “the destruction of the Ideal Theory” was what constituted “the confutation of the reasoning of BERKELEY and HUME.” Dr REID was perfectly aware that the word *Idea*, in that

argument, *might* be used only metaphorically, as asserted by Dr BROWN; and his answer to the argument is expressly so stated as to be equally applicable, *whether the word is used in the literal or the metaphorical sense*. His main argument is directed, not necessarily against the supposition of intermediate existences, called Ideas, but against the supposition that the material world, if it exists, *must be the express image or representation* of the mental acts by which we are made acquainted with it.

It will be observed, that there is no *absurdity* in saying that a Sensation, or any other mental act, uniformly attends the impression on any of our organs, made by any particular external object or quality, that it indicates to us its existence, and suggests to us, or enables us to form, a notion of its nature. The absurdity lies only in supposing, that any mental act can be the exact image or representation of anything but another mental act, in the same or another mind; and Dr REID was at pains to point out that his reply to this is independent of any particular meaning, and even of the use, of the word Idea.

He says,—“To prevent mistakes, the reader must be reminded, that if by Ideas are meant only the acts or operations of our minds in perceiving, remembering, or imagining objects, I am far from calling in question the existence of *those acts*; we are conscious of them every hour of life, and I believe no man of a sound mind ever doubted of their existence.”—(*Intellectual Powers*, p. 197.)

This shews that he was aware that the term *Ideas might* be used *metaphorically*, “or as illustrative language” for acts or states of mind.

Then he says, in stating his argument against Bishop BERKELEY,—“That we have many Sensations by means of our external senses, there can be no doubt; and if he is pleased to call these Ideas, there ought to be no dispute about the meaning of a word.” “But,” says Bishop BERKELEY, “by our senses we have knowledge *only* of our Sensations or Ideas, call them which you will; and these, which are attributes of Mind, can have no resemblance to any qualities of a thing which is inanimate. *I allow him to call them which he will*, but I would have the word *only* in this sentence to be well weighed, because a great deal depends upon it. For if it be true that by our senses we have the knowledge of our sensations *only*, then his system must be admitted, and the existence of a material world must be given up as a dream.”—(*Collected Works*, p. 290.)

Then he goes on to give the proof, that the mental act in question, however rapid, is more complex than it had been represented,—that our minds are so constituted as to form uniformly certain definite *notions on occasion of certain sensations being excited in us*,—to draw certain inferences, or pass certain judgments, as to the existence and certain qualities of things external to ourselves,—that it is to these *perceptions* that the intuitive belief of independent existence is attached, —and that these we at once perceive, when our attention is fixed on them, to be *essentially distinct from the sensations, and to resemble them in no particular*. This per-

ceived or felt *dissimilarity* of the Notions or Conceptions, as to external existences, which are formed in the mind, from the Sensations which suggest or introduce them into the mind, is what both REID and STEWART relied on, as the answer to the sceptical argument of HUME and BERKELEY; and is not once noticed either by Dr BROWN or Lord JEFFREY.

This argument is given at more length by REID as follows:—"It is true we have feelings of touch, which every moment present the notion of Extension or Space to the mind: but how they come to do so is the question; for those feelings do *no more resemble extension, than they resemble justice or courage*; nor can the existence of extended things be inferred from those feelings, by any rules of reasoning; so that the feelings we have by touch can neither explain how we get the notion, nor how we come by the belief, of extended things.

"What hath imposed upon philosophers in this matter is, that the feelings of touch, which suggest primary qualities, *have no names*, nor are they ever reflected upon. They pass through the mind instantaneously, and serve only to introduce the notion and belief of external things which, by our constitution are connected with them. They are natural signs, and the mind immediately passes to the thing signified, without making the least reflection upon the sign, or observing that there was any such thing."

"Let a man press his hand against the table, *he feels it hard*. But what is the meaning of this? The meaning undoubtedly is, that he hath a certain feeling of touch, from which he concludes, without any reasoning, or comparing ideas, that there is something external really existing, whose parts stick so firmly together, that they cannot be displaced without considerable force.

"There is here a feeling, and a conclusion drawn from it, or some way suggested by it. The hardness of the table is the conclusion, the feeling is the medium by which we are led to that conclusion. Let a man attend distinctly to this medium and to this conclusion, and he will perceive them to be *as unlike* as any two things in nature. The one is a sensation of the mind, which can have no existence but in a sentient being, nor can it exist one moment longer than it is felt; the other is in the table, and we conclude, without any difficulty, that it was in the table before it was felt, and continues there after the feeling is over. The one implies no kind of extension, nor parts, nor cohesion; the other implies all these. Both, indeed, admit of degrees, and the feeling, beyond a certain degree, is a species of pain, but adamant hardness does not imply the least pain."—(*Collected Works*, p. 125.)

The substance of this argument is, that the external existences, or qualities of external objects, of which our knowledge is acquired by the senses, are *not* felt or apprehended by us as prototypes or patterns of the sensations, through which they are made known, *but perceived to differ from them in every particular*; as in the case of the notion of Extension or Space, formerly mentioned,—formed during the exercise of various senses, *i. e.*, in consequence of the excitement of various

sensations, but which is no sooner apprehended than it "swells in the human mind to Infinity," to which notion certainly no human sensation can bear any resemblance; and no one has rightly apprehended the argument, or can be aware of the importance ascribed to it by Mr STEWART, as opposed to what has been since called the Sensational School of Metaphysicians, who has not adverted to this absolute and essential *dissimilarity* of the sensations, from what Dr REID calls "the Perceptions," and Dr BROWN, the "Associations and Inferences," consequent on those sensations. Those who do advert to that dissimilarity must perceive that our conception of, and belief in, the external and independent existence of space and matter,—although a mental act, and a complex one, and involving one of those intuitive judgments, as to the existence and authority of which we have seen that REID, STEWART, and BROWN, are fully agreed,—is perfectly distinct from the sensation by which it is excited, and involves no such *absurdity* or contradiction in terms, as the assertion that a sensation or other mental act, can be the *exact image and representation* of anything that is not mental; and therefore, that the sceptical argument of BERKELEY and HUME, founded on that supposed absurdity, and *necessitating* our *departure*, as HUME expressed it, from the instincts of nature, as to the evidence of the senses, falls to the ground.

The same observation applies to the notice of this subject by MORELL, in his review of the Scottish Philosophy, who says, that Dr REID "does not appear to him to have dealt a complete and effective blow against HUME's argument respecting the material world;" because, he says, "the sceptic may urge, with no little force, that although we must admit the reality of our own personal or subjective ideas (*i. e.*, of the objects of consciousness), yet it still remains to be proved, that our perceptions, however clear, and our beliefs, however strong they may be, internally, have reference to any object out of, and distinct from ourselves." REID, he says, deprived himself of the "power of answering this final argument, by maintaining that Perception is altogether an act of Mind. So long as perception is regarded as only a subjective process (*i. e.*, an act of mind of which we are conscious), and an idea defined to be the act of the mind in making itself acquainted with external things, we are unable to point out to the sceptic what he demands, viz., a clear passage from this subjective activity of the mind to the outward and material reality."—(*Morell's Philosophy*, vol. i., p. 287.)

Now, if this author had rightly comprehended the argument of REID,—which I apprehend he must have known only from the account of the controversy given by Dr BROWN,—he would have known that REID considered the clear passage from the act of Perception in the mind to the material reality, to be precisely similar to the passage from our consciousness of to-day to our recollections of yesterday; *i. e.*, to rest on one of those principles of Intuitive belief, the *existence* and *authority* of which are admitted by himself and by BROWN, as well as by REID; and to be from its own nature incapable of any other proof.

But if he had rightly comprehended the argument of HUME and BERKELEY, he would have known, that they not only demanded a clear passage from the mind to the material object, but maintained that it is *absurd* to assert that any such passage exists; because, as we have seen, they said that by our senses we have the knowledge *only* of our Sensations or Ideas, call them which we will, and nothing can possibly resemble a sensation, except another sensation in the same or another mind; to which assertion and consequent imputation of absurdity it was that Dr REID opposed *the fact* in the natural history of the mind, that by our senses we have the act of Perception excited in our minds, involving, as all admit, an intuitive belief; and which, particularly in the case of the primary qualities of matter, is distinctly felt by us to be separate from the sensation by which it is excited, and utterly incapable of comparison with it.

But it is equally obvious, that this perception and belief, being regarded as an ultimate fact, or as containing in itself an ultimate fact in our mental constitution, like every other *ultimate fact, physical or moral*, involves a *mystery*; and one on which we must accustom our minds to dwell, if we would form to ourselves any clear notions as to the constitution of the human mind, or its connection with the Divine Mind. It is only by a kind of Instinct, as expressed by D'ALEMBERT, but it seems better to use the term Intuition,—“prior to Reason, and superior to reason,—that the human mind can overleap the gulf that separates the visible world, from the percipient soul.”

I have already shewn that by the admission of Dr BROWN himself, in all departments of human knowledge, we meet with such ultimate facts and principles of intuitive belief, any farther explanation of which can be given us only by “the great teacher, Death;” and very little reflection is sufficient to shew that the only objects which we can propose to ourselves in any inquiry which lies on the confines of Matter and Mind,—in which both physical changes and mental acts are concerned,—are to ascertain the exact phenomena on each side of the line of demarcation, the precise conditions under which they take place, and the precise laws by which they are determined,—the mode of union being beyond our comprehension. But so restricting our objects of inquiry, we may confidently assert, that enough has been ascertained in regard to the mental operations consequent on the impressions on our senses, as well as to their physical conditions, to form an important body of science, and furnish conclusions of the highest interest.

I think myself justified by what has been stated, in affirming that in so far as Dr BROWN thought he had detected an essential error in the reasonings of Dr REID on this subject, he had deceived himself; and that in so far as he made a real advance, in our knowledge of the manner in which the notion of the primary qualities of matter is formed in the human mind, he proceeded strictly in accordance with the principles of REID and STEWART; and therefore, that it is only

retarding the progress of knowledge on the subject, to represent these authors as at variance with one another. In fact, it appears to me, that his doctrine on this subject, referring to the general Law of Belief in the permanence of the order of Nature, is substantially the same as that of STEWART and TURGOT, and that the only real addition which he made to our knowledge on the subject, consists in explaining the province of the *muscular sensations*, as distinguished from those sensations that result merely from *impressions on the cutaneous nerves*, with which they had generally been confounded under the name of Sensations of Touch; and in connection with them, the importance of *the idea of Time*, in communicating the information on which our notions of the Primary qualities of Matter are founded. This is the same distinction as is expressed by several French physiologists by the terms Tact and Toucher; and it appears from the learned researches of Sir WILLIAM HAMILTON, that it had been clearly pointed out by various other authors, ancient and modern; but I have no doubt that it was original on the part of Dr BROWN.

In concluding these remarks on this part of the Philosophy of Dr BROWN, I see no objection to my stating, what I am very certain was the case, that the repugnance which he felt towards the peculiar doctrines of Dr REID, was in reality not so much on the score of judgment as of *taste*. His own taste in literature was peculiar,—it was founded in a great measure on the classical models,—and he was even more ambitious than Mr STEWART, of combining the reputation of a scholar and elegant writer with that of an acute metaphysician. The perfect simplicity of the language, the total absence of fancy, and the homeliness of many of the illustrations, in the writings of Dr REID, were distasteful to him; and I cannot but consider, therefore, his objections to the doctrines there laid down, as an illustration of the truth of the observation on his own scientific character, which I have often heard from my Father; who had the highest admiration, both of the acuteness of his intellect, and of the purity and elevation of his moral principles, but used to speak of him as the man of the most fastidious taste that he had ever known.

It has been often observed, that the intellectual opinions, even of the men who take most pride in the exercise of their understandings, are very often more or less guided by their tastes and feelings; and in regarding the prejudice which may be detected in the writings of Dr BROWN, against the phraseology and the doctrines of REID, as an instance of the reaction of independent thought against mere authority, and of cultivated taste against the imputation of vulgarity, I do not think I do injustice to the memory of either of these illustrious men.

Sir WILLIAM HAMILTON, as I already mentioned, expresses himself strongly as to the doctrine of REID regarding the formation of the notion of the primary qualities of matter, as so far from “being a confutation of Idealism, affording it the best of all possible foundations;” but then he explains this by saying that he

means only "that simpler and more refined Idealism, which views in ideas only modifications of the mind itself," *i. e.*, only what Dr BROWN, in one passage already quoted, regarded the ideas of BERKELEY, *viz.*, as a metaphorical way of expressing acts or states of the mind; in which sense Dr REID, as we have already seen, said he did not object to the use of the term, although he preferred another phraseology; and using it in that sense, we have seen that his argument against HUME and BERKELEY is independent of any objection to the term.

Dr REID goes no farther in explaining the manner in which we acquire the knowledge of extension or space, than to say, that it is a Perception, or a notion suggested to the mind by certain of our sensations, distinctly formed in the mind, and in which, when we analyse it as minutely as we can, we find the belief of external independent existence to be an essential element. Sir WILLIAM HAMILTON considers the conception of Space to be a native form, or necessary condition of thought; but that we have an immediate perception of something extended, *i. e.*, invested with this quality, and which is independent of us. (See Notes to pages 126 and 324 of *Collected Works*, &c.)

I cannot perceive that there is anything more than a verbal distinction between these forms of expression; but if there be a real improvement in the latter form, it seems to me that it is sufficiently provided for by Dr REID's admission, that a finer eye may trace the labyrinth farther than he has done; but that in the meantime "there is no harm done" in resting on the position of REID as to that belief; and acquiescing in his reflection, that "if we are deceived in it, we are deceived by Him that made us, and there is no remedy."

It is stated by MORELL, and I believe is the opinion of others who have made a study of recent German works on metaphysics, that the works of Dr REID and all other Scotch metaphysicians, although accurate, so far as they go, in "investigating and classifying the more obvious phenomena of the mind, as they appear in the individual, are deficient in not having gone a step farther, and discovered the very laws of our mental constitution, on which our primitive beliefs rest; that they might have sought the groundwork of our universal notions in the depths of our own being, and thus referred all the principles of common sense, all the primary laws of belief, back to their source in the subjective forms of the understanding and the reason (*Historical and Critical View*, &c., vol. ii., p. 64); that in investigating the mental phenomena, our object should be to discover, not merely the reality of certain principles, but their necessity,—not merely the law of operation, but the reason of that law" (*Ditto*, p. 53); and that this is to be done, not by mere induction, but "by scanning the contents of our consciousness by the power of reflection, whereby we are enabled to catch the very forms of our inward activity."—(P. 52.)

In forming this opinion, I cannot help thinking that this very learned and estimable author has deceived himself; and that no such advance has been made

since the time of STEWART and BROWN, either in the mode of inquiry, or in the results of inquiry on the subject. But all that I wish to observe on that point is this, that those speculations avowedly relate to subjects *ulterior to those* on which REID and STEWART exerted their minds; that they do not *stand opposed* to the doctrines of REID or STEWART as to the exercise of the senses, and the mental acts thence resulting, but are regarded as an addition to these doctrines; and therefore, that, whether admitted or rejected, they ought not to interfere with our appreciation of the truth or importance of the principles regarding our mental constitution, which they had laid down, and which these authors substantially approve.

In particular, while I cannot but admire the sublimity of the Theological inferences which MORELL has stated as resulting from the study of the Mind as he directs it, I cannot think it necessary to go farther into the subject than REID and STEWART had done, in order to draw from it inferences as satisfactory to the intellect, and as consoling to the heart of man, as can be drawn from any unassisted human contemplation or reflection.

It is stated, indeed, by MORELL, that the great argument of Natural Theology, drawn from the observed adaptation of means to ends,—of which I may observe, that the principle of the adaptation of the construction of animals to the *conditions of their existence*, so well illustrated since their time by CUVIER, OWEN, and their followers, is distinctly an example,—has been well set forth by all the Scottish School of Metaphysicians, from REID to CHALMERS; but that two subjects connected with it ought to have been taken up more fully, viz., 1st, the origin of the idea of Absolute Power, or of the Divinity in the mind; and, 2^d, the relation of the Divine Power, or Energy, to Man on the one hand, and to Nature on the other.—(*Modern Philosophy*, vol. ii., p. 71.) The first of these, I think, may really be regarded as a defect in the philosophy of Dr BROWN, who rested the great argument of Natural Theology *exclusively* on the observed adaptation of means to ends;—and did not admit as a part of that argument, the formation of the notion of Efficient Cause, as distinguished by REID and STEWART from Physical Cause;—and that it was a defect seems to me distinctly shewn by an observation of his own, which I cannot reconcile with the doctrine which he had laid down on this subject.

The passage to which I allude is that where he speculates, with his usual eloquence and fancy, on the emotions which would be excited in the human race if it were possible that they should come to maturity in a world of darkness, and the sun were then suddenly to arise on their sight. “The very atheists of such a world,” he says, “would confess that there is a Power that can create.” Now he surely could not have maintained that this instantaneous inference would imply a process of reasoning, by which the supposed atheists might satisfy themselves that some particular object was in view, which could only be attained by an influence of the sun, and therefore saw in this sudden and striking change an adaptation of

means to ends. If not, then this inference must be allowed to establish the fact of the observation of *sudden and striking change* introducing into the mind the notion of a "Power that can create;" and I cannot conceive that the notion arising in the mind from the contemplation of these circumstances, and which is here expressed by that term, excludes the idea of Arbitrary Will. If it includes that idea, it cannot be correctly expressed by the definition given by Dr BROWN of Power, which he allowed to be a simple idea, formed by intuition, but was at great pains to prove to mean merely "Invariable Sequence, having reference not only to the past, but to every future case." (*Observations on Cause and Effect*, p. 101.) I cannot help thinking, therefore, that this illustration is sufficient to establish the reality of the idea of Absolute Power, or of Efficient Cause, as distinguished from Physical, which was maintained by REID and STEWART, but contested by BROWN. This criticism of MORELL, therefore, I believe to be justly applicable to BROWN, but certainly not to either of his predecessors.

The second principle stated by MORELL as having been neglected by the Scottish School of Metaphysicians, is so beautifully expressed by himself, that I cannot help quoting his words. The principle in question, he says, should be a comment "on the scriptural doctrine, that in God we live and move and have our being. This is a truth which has more meaning in it than the cursory reading of it gives us; it evidently has a reference to the mysterious dependence of the human spirit upon the Divine, shewing us that we are all emanations from the Divine Essence, and although gifted with a distinct personality, yet that we are but waves in the great ocean of existence, ever rolling onwards to our eternal home."—(MORELL, vol. ii., p. 72.)

Now if the doctrine of REID and STEWART really excluded from the reflections of the metaphysician so elevating and consoling a train of thought as this, we might regard them as truly and lamentably defective; but I confidently maintain, that all that is necessary is to let the mind dwell for a little on the principle of Intuitive Perception of Truth, illustrated by them as well as by BROWN, and in connection with it, on the facts regarding our mental constitution which they have explained, in order to be satisfied of the truth and justice of the sentiment which I have quoted, and which, indeed, in all ages has suggested itself to the most profound thinkers in this department of science.

"Intuition or Inspiration," says VICTOR COUSIN, "is in all languages distinct from reflection or from Reasoning. It is the simple perception of Truth; I mean of essential and fundamental truths, without the intervention of any voluntary or personal act. This intuition *does not belong to us*. We are there, when the act is performed in our minds, simply as spectators, not as agents; all our action consists in having the consciousness of what is going on. Our perception of simple and primary truths may be separated, therefore, from the fallible reason of man, and referred to that Reason which is Universal, Absolute, Infallible, and Eter-

nal, beyond the limits of Space and Time, above all contact with error or disorder,—to that Intelligence of which ours, or that which makes its appearance in us, is but a fragment,—to that Mind, pure and incorruptible, of which ours is only the reflection.”

These are sentiments which adorn and dignify Science, but I beg to ask, whether they are not in exact accordance with the doctrines of all our esteemed Scottish metaphysicians,—nay, whether they may not be regarded as commentaries on the simple text already quoted from REID, that all our knowledge of what exists, or ever did exist, traced to its source, is found to come by a channel, which is open to those who cannot reason, *i. e.* (the word reason being ambiguous), who cannot exert the voluntary power of Reasoning, but only yield to the influence of the faculty of Intuition implanted in their nature,—“that we are led to it in the dark, and know not how we came by it,—and that the wise and humble will simply take it as the gift of Heaven, and try to make the best use of it.” According to the doctrine of REID, all those mental acts in which Intuitive Belief is involved, and on which all knowledge is directly or indirectly founded, although we call them *ours*, are ultimate facts in Nature, independent of our will, and beyond our comprehension; and this conclusion, so far from humbling the human mind, establishes a more intimate connexion between man and his Creator than can be inferred from any other facts in nature.

When we attend to the meaning, and trace the applications of this principle of Intuition, necessarily involved in the only account we can give of our perceptions, and of all our knowledge; when we observe the still more striking exercise of this power in animals, whose sensations suggest to them, prior to all experience, the true distance, direction, and size of external objects, certainly neither contained in, nor deducible by any process of reasoning from, the intimations of sense; when we reflect on the equally mysterious nature, and yet on the proved fidelity (in the healthy state) of the evidence of Memory, essential, not only to all reasoning, but to all definite voluntary action of men and animals; when we consider the nature and the tendency of those Instinctive propensities or Impulses, which are excited in us and in all animals during the exercise of the senses, and which are equally requisite and equally effective, in attaining objects essential to our existence, as are the vital properties of muscles and of nerves;—in all these cases, we shall perceive that truths are made known to us in a manner *absolutely mysterious*;—by means of impressions on our senses, but “no more contained in sense, than the explosion of a cannon in the spark that gave it fire.”

And when we farther observe, that the *actions* which are prompted by the Instincts and Volitions both of animals and of men, consequent on the knowledge thus acquired, are all conducive to certain important ends, intelligible to us after observation and reflection, but scarcely ever in the contemplation of the agents at the moment, we can express these facts only by saying that both men and animals

are the depositaries or recipients of certain portions of the knowledge, and the instruments of certain of the designs, of the superior Mind to which they owe their existence. And the "creed of the sceptic," shewing that it is by no exertion of our own reason, and indeed by no process of which we can give any account, that so many truths are made known to us, and so many useful acts suggested to us, becomes an essential part of the short and simple train of reasoning by which that connection is inferred, and which may be thus stated.

Much of the knowledge which is part of the constitution of our minds, or which is awakened in us by the exercise of our senses, is not our knowledge; it is neither contained in our sensations, nor deducible by any reasoning from them, nor subject to our will, nor acquired by our experience or recollection; yet it is found to be accurate, and the possession of it to be useful and necessary to us.

So also, many of the actions which we perform, which are fitted to the attainment of ends important to us, and obviously performed in anticipation of those ends, are not prompted by any such anticipation of ours. The will which performs them is ours, but the knowledge of their consequences, with a view to which they are performed, is not ours. "Man," says GUIZOT, "is a workman, intelligent and free, but the work in which he is employed is not his; he sees the intention of it only when it has been so far accomplished, and even then, sees it only imperfectly." In so far, therefore, as the observation of these phenomena of our minds leads to an inference of Intelligence,—and if it does not, we have no grounds for ascribing intelligence to any of our friends or fellow-citizens,—it must be intelligence prior to ours, and superior to ours, and on which ours is dependent.

It seems to me, that it is quite unnecessary to make any additions to the doctrine, which we have seen was the common doctrine of REID, of STEWART, and of BROWN, as to the existence and authority of the Intuitive Principles of Belief,—and hardly necessary to illustrate this farther than the two former authors had done,—to justify the whole of this inference. But farther, it is precisely the same inference which we find, if not so fully illustrated, at least distinctly expressed as resulting from the contemplation of our mental constitution, by much earlier authors. It was the same idea that was expressed by the three memorable words of CICERO, "*Homo Rationis Particeps*" (not possessor); and by the positive assertion of PLATO,—that nothing is more certain than that a part of every man's mind existed before he did. Nay, in an earlier record than either of these, of the first metaphysical reflections of the human race, in those very words from the Book of Job which Dr REID took as the motto of his Work on the Intellectual Powers, there is, as we are assured by an eminent Hebrew scholar, a meaning more exactly in accordance with the leading principle of Dr REID's Philosophy, than, in selecting that motto, he was probably aware. The words are, "Who hath put wisdom in our inward parts?" but the more precise expression of the meaning, we are assured, is,

Who hath given to our inward parts, or to our thoughts, the *security of knowledge*? i. e., What security have we of the truth or reality of knowledge, which we can trace no farther than certain impressions made on, and changes excited in, our own minds? and the only answer which the context will admit is, that we have no security but the will of our Maker, whereby our minds are so constituted, that Belief is an essential component part of the acts which they uniformly perform, or the states which they uniformly assume, under certain circumstances; which in this as in other departments of knowledge, we can go no farther than to specify and describe.

I may just add, that there are two questions in Physiology, which have attracted much attention of late years, and of which I think a just view cannot be taken, without a previous accurate discrimination of those mental phenomena which Dr REID distinguished as Sensations, Perceptions, Recollections, and Voluntary Efforts. The first regards the appropriation of the larger masses of the nervous system to their specific uses; and *first*, to those muscular movements which are generally now described as depending on the Reflex action of the Spinal Cord, *e. g.*, those concerned in Respiration, Deglutition, and the various actions associated with those, and which have been ascertained, particularly by the experiments of FLOURENS, to have no dependence on the hemispheres of the Brain or Cerebellum; and, accordingly go on, even for months together, in animals of which both the brain and cerebellum have been extirpated; so that the term Reflex Spinal Action may be properly applied to them, instead of the older term Sympathetic Action, by which they were long previously distinguished. But it is equally certain, and was indeed established long ago, by Dr WHYTT, that another principle is here concerned, which goes so far in explanation of the fact, not only that muscular contractions are excited by this reflex action in these circumstances, but that those muscles are *selected* for this purpose, which are capable of performing the motions, and successions of motion, requisite for the particular end to be attained in each case,—one set of motions, *e. g.*, for breathing, another for coughing, another for deglutition, another for vomiting, &c. That principle is the existence and the peculiarity of the Sensation, preceding and attending the performance of each of these motions. The proof of this is, that in many of these cases, the same sensation may be excited by impressions made on the sensitive nerves of different parts, in each of which the same reflex or sympathetic movement follows; while in others, different sensations result from varied impressions made on the sensitive nerves of the same parts, and in these different reflex actions are excited. It appears, therefore, that it is by the sensations preceding and attending them, that the nature and intensity of these reflex movements are determined, at least in the ordinary exercise of these functions; and that those parts of the nervous system, and those only, which are found to be essential to those movements, must be those which are concerned

in the mental act of Sensation; which term is now habitually used in Physiology, in exactly the same sense as Dr REID understood it.*

Accordingly, I think it may be confidently asserted,—although many physiologists speak of reflex actions as not necessarily connected with sensation,—that the correct expression of these phenomena was truly given by CUVIER, in his Report to the Academy of Sciences on the Memoir of FLOURENS in 1822,—that an animal of which brain and cerebellum have been destroyed, and the medulla oblongata only remains in the cranium, is still capable of feeling Sensation, and of performing those acts which are immediately linked with sensation; and, indeed, is dependent on sensations for the preservation of its life, which, in these circumstances has been preserved for many months,—because it still breathes, and still swallows what is put into its mouth, &c.; but that, in these circumstances, it has *no recollection of past sensations*, shews none of its usual habits, cannot seek for food, or even avoid obstacles placed in its way; in short, is reduced to a state of stupor, more or less profound. In such an animal, of course, those judgments consequent on sensations, to which both Dr REID and Dr BROWN gave the name of Perceptions, and all more strictly Mental recollections and acts consequent on these, are manifestly suspended; and thus we acquire the certainty that the distinction of Sensations and Perceptions, which we have seen to be of so much importance when considered metaphysically, is fully confirmed by physiological inquiries, and, I may add, by researches in Comparative Anatomy; which have proved that the Cerebro-Spinal Axis is the part of the animal structure which furnishes the conditions, and supplies the instrument, of the ones et of mental phenomena; and the Brain and Cerebellum, superimposed on that structure within the skull, are those which minister in like manner to the other. This is, in fact, the only conclusion, as to the appropriation of these different parts of the larger masses of the nervous system to different acts or states of mind, which has yet been satisfactorily established; and if we regard it, as I think we may, as an important guide to farther inquiries as to the use of the different portions of the physical instrument concerned in Thought, we ought also to regard it as an important indication of the value of the distinctions among the acts of thought, with which these different portions of the nervous system are connected.

The other question is, as to the degree of modification which the exercise of the Senses, as well as other mental acts may undergo, in several anomalous conditions of the living body, especially in that to which the term Somnambulism, Extâse, or Clairvoyance, has been applied. On this subject, which can only be elucidated by very carefully-conducted observations,—always likely to be impeded by peculiar

* See Observations on the Physiological Principles of Sympathy, by the present Author, in Edinburgh Medico-Chirurgical Transactions, vol. ii.

sources of fallacy, especially by that extraordinary propensity to deception which medical men so continually encounter in this part of their studies,—it would be wrong in me, not having had sufficient opportunities for making such observations, to pronounce any decided opinion; but I think it only due to the memory of Dr REID to point out, that in one part of his writings he has distinctly asserted, —and indeed, consistently with his principles, could not fail to perceive,—the possibility of such a modification of the exercise of the senses, as has been expressed by the term Clairvoyance; and left it, therefore, as a question to be decided simply by experience, whether or not such modification may occur.

“Our power of perceiving external objects is limited in various ways, and particularly in this, that without the organs of the several senses, we perceive no external object. We cannot see without eyes, nor hear without ears; and it is not only necessary that we should have these organs, but that they should be in a sound and natural state.

“All this is so well known from experience, that it needs no proof; but it ought to be observed, that we know it *from experience only*. We can give no reason for it, but that such is the will of our Maker. No man can shew it to be impossible for the Supreme Being to have given us the power of perceiving external objects without such organs.

“If a man were shut up in a dark room, so that he could see nothing but through one small hole in the shutter of a window, would he conclude that the hole was the cause of his seeing, and that it is impossible to see in any other way? Perhaps, if he had never in his life seen but in this way, he might be apt to think so; but the conclusion is rash and groundless. He sees, because God has given him the power of seeing; and he sees only through this small hole, because his power of seeing is circumscribed by impediments on all other hands.”—(REID's *Collected Works*, p. 246.)

On this passage we have the following note by Sir WILLIAM HAMILTON:—
“However astonishing, it is now proved beyond all rational doubt that, in certain abnormal states of the nervous organism, perceptions are possible through other than the ordinary channels of the senses.”

This is expressing a decided opinion on the question, on which I have said that I do not think myself qualified to judge; but I beg to express my perfect concurrence with Sir WILLIAM HAMILTON in thinking, that, consistently with the principles of Dr REID, it is a question on which no *à priori* opinion is admissible, and which observation and experiment alone can decide.

XXXIV.—*Summation of a Compound Series, and its Application to a Problem in Probabilities.* By BISHOP TERROT.

(Read 21st February 1853.)

The series proposed for solution in the following paper is—

$$\left. \begin{aligned} &(\overline{m-q} \cdot \overline{m-q-1} \dots \overline{m-q+p+1}) \times (1 \cdot 2 \cdot 3 \dots q) \\ &+ (\overline{m-q-1} \cdot \overline{m-q-2} \dots \overline{m-q+p}) \times (2 \cdot 3 \cdot 4 \dots q+1) \\ &\vdots \\ &+ (\overline{p \cdot p-1} \dots 1 \times (\overline{m-p} \cdot \overline{m-p+1} \dots \overline{m-p+q+1})) \end{aligned} \right\} \quad (A)$$

The law of this series is manifest. Each term is the product of two factorials, the first consisting of p , and the latter of q factors. And in each successive term, the factors of the first factorial are each diminished by one, and those of the latter increased by one.

Let there be a series, $X_n Y_1 + X_{n-1} Y_2 + \dots X_1 Y_n$.

where, $Y_2 = Y_1 + \Delta_1$, $Y_3 = Y_2 + \Delta_2 = Y_1 + \Delta_1 + \Delta_2$, and so on.

Then the series = $X_n \times Y_1$

$$+ X_{n-1} \times \overline{Y_1 + \Delta_1}$$

$$+ X_{n-2} \times \overline{Y_1 + \Delta_1 + \Delta_2},$$

&c.

$$= \Sigma X_n \times Y_1 + \Sigma X_{n-1} \times \Delta_1 + \Sigma X_{n-2} \times \Delta_2 + \&c.$$

where ΣX_n means the sum of all the terms of X from X_1 to X_n inclusive.

Let us then, in the first place, take the differences of the second factorials—

$$-(1 \cdot 2 \cdot 3 \dots q) + (2 \cdot 3 \cdot 4 \dots q+1) = (2 \cdot 3 \cdot 4 \dots q) \cdot q$$

$$-(2 \cdot 3 \cdot 4 \dots q+1) + (3 \cdot 4 \cdot 5 \dots q+2) = (3 \cdot 4 \cdot 5 \dots q+1) \cdot q$$

&c.

&c.

Hence the sum of the whole series =

$$\left. \begin{aligned} &\Sigma (\overline{m-q} \cdot \overline{m-q-1} \dots \overline{m-p+q+1}) \cdot 1 \cdot 2 \cdot 3 \dots q-1 \cdot q \\ &+ \Sigma (\overline{m-q-1} \cdot \overline{m-q-2} \dots \overline{m-p+q}) \cdot 2 \cdot 3 \cdot 4 \dots q \cdot q \\ &+ \Sigma (\overline{m-q-2} \cdot \overline{m-q-3} \dots \overline{m-p+q-1}) \cdot 3 \cdot 4 \cdot 5 \dots q+1 \cdot q \\ &\quad \&c. \quad \&c. \end{aligned} \right\} \quad (B)$$

Integrating then each line separately, we have the sum

$$\left. \begin{aligned} &= \frac{q}{p+1} \cdot m-q+1 \cdot m-q \dots m-\overline{p+q}+1 \times 1.2.3 \dots q-1 \\ &+ \frac{q}{p+1} \cdot m-q \cdot m-q-1 \dots m-\overline{p+q} \times 2.3.4 \dots q \\ &+ \frac{q}{p+1} \cdot m-q-1 \cdot m-q-2 \dots m-\overline{p+q}-1 \times 3.4.5 \dots q+1 \\ &\quad \&c. \quad \quad \quad \&c. \end{aligned} \right\} \quad (C)$$

If again we treat this form as we have done the original, by taking the differences of the second factorials as they now stand, and again integrating, we reproduce the sum in the form

$$\left. \begin{aligned} &\frac{q \cdot q-1}{p+1 \cdot p+2} \cdot m-q+2 \cdot m-q+1 \dots m-\overline{p+q}+1 \times 1.2.3 \dots q-2 \\ &+ \frac{q \cdot q-1}{p+1 \cdot p+2} \cdot m-q+1 \cdot m-q \dots m-\overline{p+q} \times 2.3.4 \dots q-1 \\ &\quad \&c. \quad \quad \quad \&c. \end{aligned} \right\} \quad (D)$$

It appears, then, that we may continue this differentiation on the one side q times, and integration on the other $q+1$ times; and that at each succeeding operation, an additional next lower factor will be introduced into the numerator of the fractional coefficient, and an additional next highest into the denominator. And after q differentiations, the last factorials will all become unity; and, the middle factorial having acquired an additional higher factor at each of $q+1$ integrations, we have for the sum of the series—

$$\frac{q \cdot q-1 \cdot q-2 \dots 1}{p+1 \cdot p+2 \dots p+q+1} \times m+1 \cdot m \dots m-\overline{q+p}+1 \quad \left. \right\} \dots (E)$$

II.

The Problem in Probabilities to which the foregoing summation is applicable, is the following:—

Suppose an experiment concerning whose inherent probability of success we know nothing, has been made $\overline{p+q}$ times, and has succeeded p times, and failed q times, what is the probability of success on the $\overline{p+q+1}^{\text{th}}$ trial.

This Problem is interesting, because it tends to the discovery of a rational measure for those expectations of success which constitute the motive for a large portion of human actions. The force of such expectations commonly depends, not upon reason, but upon temperament; and, according, as a man is naturally sanguine or the reverse, so in all the contingencies of life, does he over-estimate or under-estimate the chances in his favour.

It would be going much too far to think, that we can give an algebraic formula, by the application of which a man may, in every practical case, correct his natural tendency to error, and arrive at a strictly rational amount of expectation.

All that we can say is, that experience has led dispassionate men to come to nearly the same conclusion as the mathematician: for while he asserts the probability of success to be $\frac{p+1}{p+q+2}$, they act upon the supposition that the probabilities of success and failure are proportioned to the number of experienced cases of success and failure: and when either p or q is a large number, that is, when the experience is great, the conclusion and the supposition coincide.

In order to realise the Problem, we shall use the ordinary illustration, and suppose that a bag contains m balls in unknown proportions of black and white, but all either black or white; that p white and q black balls have been drawn, and that it is required to find the probability of drawing a white at the $p+q+1^{\text{st}}$ drawing.

The Problem as thus stated, admits of four varieties.

1. m may be given, and the balls drawn may have been replaced in the bag.
2. m may be given, and the balls drawn not replaced.
3. m may be infinite or indefinite, and the balls replaced.
4. m may be infinite or indefinite, and the balls not replaced.

Of these, the 3^d is the only case which I find solved in the treatises which I have consulted. I propose to solve the 2^d case, and therein the 4th; and, in conclusion, to make an attempt at the solution of the 1st case.

To render the observed event, that is, the drawing of p white and q black balls (or E), possible, the original number of whites may have been any number from $m-q$ to p inclusive, and the number of blacks any number from q to $m-p$.

Let us call the hypothesis of $m-q$ white and q black, H_1 ,
and $m-q-1$ white and $q+1$ black, H_2 , &c.

Then H_1 gives for probability of E $\frac{m-q \cdot m-q-1 \dots m-q-p+1 \times 1 \cdot 2 \cdot 3 \dots q^*}{m \cdot m-1 \dots m-q-p+1}$
or, calling the denominator A,

$$\left. \begin{aligned} H_1 \text{ gives } \frac{1}{A} \cdot m-q \cdot m-q-1 \dots m-q-p+1 \times 1 \cdot 2 \cdot 3 \dots q & \quad (\alpha) \\ H_2 \text{ gives } \frac{1}{A} \cdot m-q-1 \cdot m-q-2 \dots m-q-p \times 2 \cdot 3 \cdot 4 \dots q+1 & \quad (\beta) \\ H_3 \text{ gives } \frac{1}{A} \cdot m-q-2 \cdot m-q-3 \dots m-q-p-1 \times 3 \cdot 4 \dots q+2 & \quad (\gamma) \\ \text{\&c.} & \quad \text{\&c.} \end{aligned} \right\} \quad (F)$$

Now, $\alpha + \beta + \gamma$, &c. by the former proposition (E)

$$= \frac{1}{A} \cdot \frac{q \cdot q-1 \dots 1}{p+1 \cdot p+2 \dots p+q+1} \times m+1 \cdot m \dots m-p-q+1$$

$$\therefore \text{probability of } H_1 = \frac{\alpha}{\alpha + \beta + \gamma, \text{ \&c.}}$$

* The coefficient (U of GALLOWAY's Treatise), expressing the number of different ways in which p white and q black balls can be combined in $p+q$ trials, is here omitted. This is immaterial, as it disappears in the expression $\frac{\alpha}{\alpha + \beta + \gamma, \text{ \&c.}}$

$$= \frac{p+1 \cdot p+2 \dots p+q+1}{m+1 \cdot m \dots m-p-q+1 \times 1 \cdot 2 \cdot 3 \dots q} \times (m-q \cdot m-q-1 \dots m-q-p) + (1 \times 1 \cdot 2 \cdot 3 \dots q)$$

But the probability of a white at $p+q+1^{\text{th}}$ drawing on H_1 is $\frac{m-p-q}{m-p-q}$

$\therefore$ probability of white derived from H_1 is

$$\frac{p+1 \cdot p+2 \dots p+q+1}{m+1 \cdot m \dots m-p-q+1 \times 1 \cdot 2 \cdot 3 \dots q} \times (m-q \cdot m-q-1 \dots m-q-p) \times (1 \cdot 2 \cdot 3 \dots q) \quad (G)$$

So probability from H_2

$$= \frac{p+1 \dots p+q+1}{m+1 \cdot m \dots m-p-q \times 1 \cdot 2 \cdot 3 \dots q} \times (m-q-1 \cdot m-q-2 \dots m-q-p-1) \times (2 \cdot 3 \dots q+1)$$

And so for all the other hypotheses in succession.

Now this series, omitting for the present the consideration of the fraction which is a factor common to them all, is a series of the same form as that summed in the last proposition, only that now $p+1$ must be substituted for p .

We have therefore the whole probability of a white at $p+q+1^{\text{th}}$ drawing

$$= \frac{p+1 \cdot p+2 \dots p+q+1}{m+1 \cdot m \dots m-p-q+1 \cdot 2 \dots q} \times \frac{1 \cdot 2 \dots q}{p+2 \dots p+q+2} \times \left. \begin{array}{l} \\ \\ \\ \end{array} \right\} \dots \dots \quad (H)$$

$$m+1 \cdot m \dots m-p-q = \frac{p+1}{p+q+2}$$

Note.—It may be worth observing, that, had we summed the original series in Prop. 1. upwards instead of downwards, we should have got for a first factor

$\frac{1 \cdot 2 \cdot 3 \dots p}{q+1 \cdot q+2 \dots p+q+1}$, which must therefore $= \frac{1 \cdot 2 \cdot 3 \dots q}{p+1 \cdot p+2 \dots p+q+1}$. And that these fractions are equal may be proved independently, for if we divide each by $1 \cdot 2 \cdot 3 \dots p \times 1 \cdot 2 \cdot 3 \dots q$, we have on both sides the same quotient

$$\frac{1}{1 \cdot 2 \cdot 3 \dots p+q+1}$$

There now remains for solution only the first case of the problem in chances, that is, to find the probability of drawing a white ball, when m the number of balls is given, and p white and q black have already been drawn and returned.

The main object in this case is to sum the series

$$m-1^p \times 1^q + m-2^p \times 2^q \dots \dots 1^p \cdot m-1^q \dots \dots \quad (I)$$

This may be done much as in the preceding case, by taking the successive differences of the right-hand factors till the differences vanish, and multiplying the successive terms of the last or $q+1^{\text{th}}$ row of differences into the $q+1^{\text{th}}$ summation of the successive terms of the series $(1+2^p \dots + m-1^p) + (1+2^p \dots + m-2^p)$, &c.

This may be sufficiently explained by going through the operation in a low particular case. Let $p=2, q=3$.

Then the series written perpendicularly is

$$\begin{array}{cccccc}
 \overline{m-1^2} \times 1 & \Sigma_1 \overline{m-1^2} \times 1 & \Sigma_2 \overline{m-1^2} \times 1 & \Sigma_3 \overline{m-1^2} \times 1 & \Sigma_4 \overline{m-1^2} \times 1 & \\
 \overline{m-2^2} \times 8 & \Sigma_1 \overline{m-2^2} \times 7 & \Sigma_2 \overline{m-2^2} \times 6 & \Sigma_3 \overline{m-2^2} \times 5 & \Sigma_4 \overline{m-2^2} \times 4 & \\
 \overline{m-3^2} \times 27 = \Sigma_1 \overline{m-3^2} \times 19 = \Sigma_2 \overline{m-3^2} \times 12 = \Sigma_3 \overline{m-3^2} \times 6 = \Sigma_4 \overline{m-3^2} \times 1 & & & & & \\
 \overline{m-4^2} \times 64 & \Sigma_1 \overline{m-4^2} \times 37 & \Sigma_2 \overline{m-4^2} \times 18 & \Sigma_3 \overline{m-4^2} \times 6 & & \\
 \overline{m-5^2} \times 125 & \Sigma_1 \overline{m-5^2} \times 61 & \Sigma_2 \overline{m-5^2} \times 20 & \Sigma_3 \overline{m-5^2} \times 6 & & \\
 \&c. & \&c. & \&c. & \&c. &
 \end{array}$$

The value of the different sigmas is easily found by the method of finite differences.

Generally, since the differences of 1^q , 2^q , 3^q , &c., always vanish in the $q+1^{\text{th}}$ line and after the q^{th} term of it, the general expression is

$$\Sigma_{q+1} \overline{m-1^q} + d_2 \Sigma_{q+1} \overline{m-2^q} \dots d_q \Sigma_{q+1} \overline{m-q^q};$$

d_1 , d_2 , d_3 , &c., signifying the 1st, 2d, 3d, &c., terms of the $q+1^{\text{th}}$ row of differences.

This summation may be applied to find the probability in the case now under consideration, for it expresses the $a+\beta+\gamma$, &c., of the preceding case. Applying it as we did the value of $a+\beta+\gamma$, &c., there found, we shall find the probability of a white ball at the $p+q+1^{\text{th}}$ trial to be

$$\frac{\Sigma_{q+1} \overline{m-1^{p+1}} + d_2 \Sigma_{q+1} \overline{m-2^{p+1}} \dots d_q \Sigma_{q+1} \overline{m-q^{p+1}}}{m (\Sigma_{q+1} \overline{m-1^p} + d_2 \Sigma_{q+1} \overline{m-2^p} \dots d_q \Sigma_{q+1} \overline{m-q^p})} \quad (K)$$

If m be infinite, the expression becomes

$$\frac{(1+d_2+\dots+d_q) \cdot \Sigma_{q+1} m^{p+1}}{m (1+d_2+\dots+d_q) \cdot \Sigma_{q+1} m^p} = \frac{\Sigma_{q+1} m^{p+1}}{m \Sigma_{q+1} m^p}$$

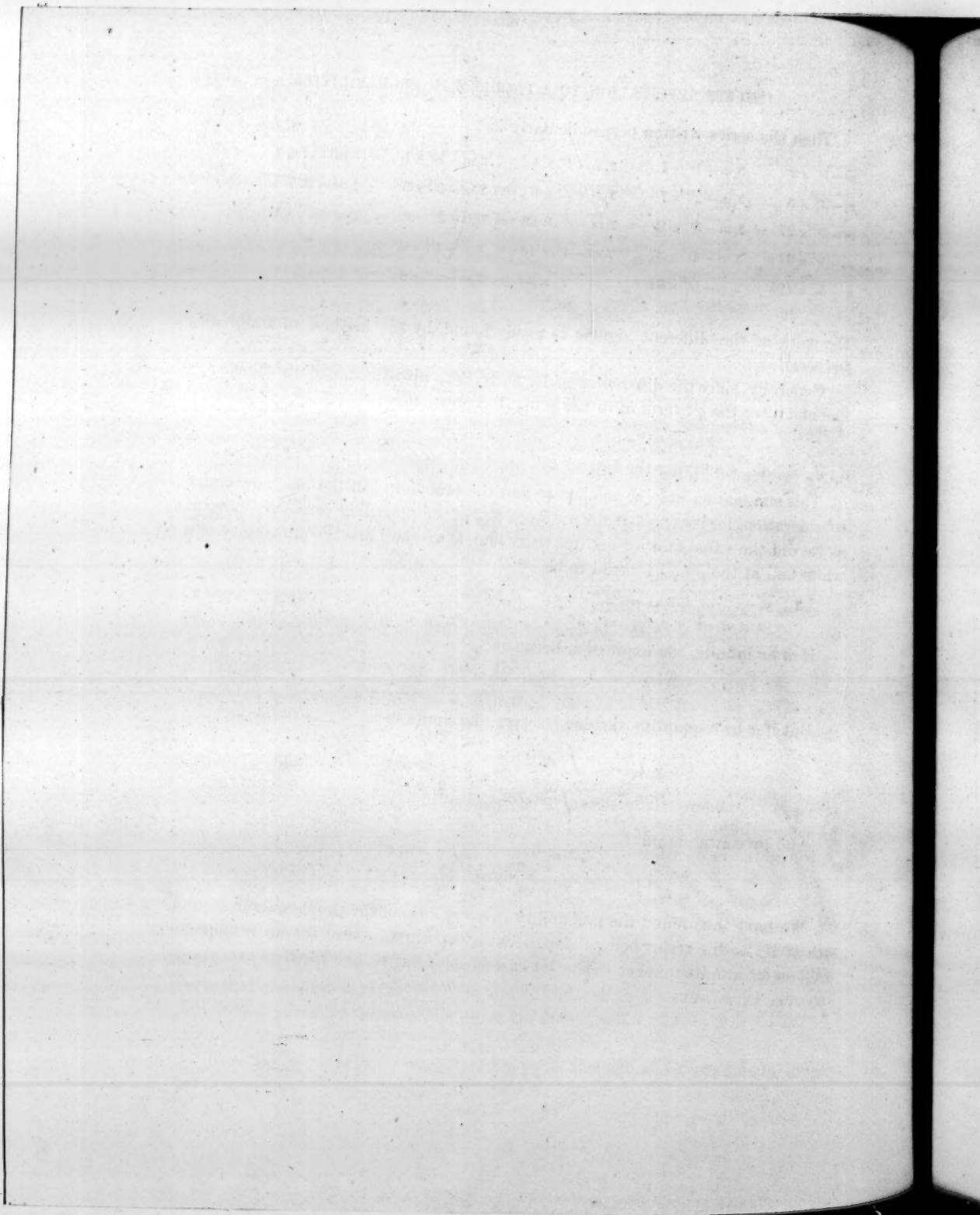
But if x be a quantity varying between the limits 0, x ,

$$\frac{\Sigma_1 m^{p+1}}{m \Sigma_1 m^p} = \frac{\int_0^x x^{p+1} dx}{x \int_0^x x^p dx} = \frac{p+1}{p+2} \cdot \frac{x^{p+2}}{x \cdot x^{p+1}}$$

And by continuation

$$\frac{\Sigma_{q+1} m^{p+1}}{m \Sigma_{q+1} m^p} = \frac{p+1 \cdot p+2 \dots p+q+1}{p+2 \cdot p+3 \dots p+q+2} = \frac{p+1}{p+q+2} \quad (L)$$

We have thus found the probability in every case of the problem; the 2d and 4th at H, for the result being independent of m , must be true for an infinite as well as for a finite number. The 1st case is solved at K, and the 3d at L.



XXXVII.—*On the Optical Phenomena and Crystallisation of Tourmaline, Titanium, and Quartz, within Mica, Amethyst, and Topaz.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., and V.P.R.S. Edin. (With a Plate.)

(Read 4th January 1853.)

The existence of certain minerals imbedded in others,—the optical phenomena which they exhibit,—their form and mode of distribution, and the mechanical influence which has been exerted during their formation on the mineral that contains them, are among the most curious and instructive facts in physical science.

The dissemination of perfectly-formed crystals of titanium, both in the form of *titanite* and *anatase*, in Brazilian crystals of quartz, is a fact so well known that I shall take no farther notice of it, but shall proceed to give an account of a series of facts of a much more general and interesting character, which I have had occasion to observe, during an extensive examination of minerals, undertaken with a different object.

1. *On the Distribution of Tourmaline in Mica.*

When fluids and condensed gases are imprisoned in the cavities of *topaz* and other hard minerals, they retain their place till some powerful agent releases them from confinement, or till heat gives them such an expansive force as to burst the mineral. In *mica*, however, where the laminæ of which it is composed are held together by a very feeble cohesive force, the fluids in their cavities, and the extraneous materials which were present at their formation, have experienced no difficulty in quitting their place, and spreading themselves between the plates of the mineral.

Tourmaline and *quartz*, though thus distributed between the laminæ of mica subsequent to its crystallisation, have yet found a place in it contemporaneously with the crystallisation of the mica itself. In this case they are large crystals, equivalent in thickness to many laminæ, and may be taken out and subjected to examination. Some of the crystals of tourmaline are so large, indeed, that I have used them with their own natural faces as analysing prisms; and the quartz crystals, which are amorphous, and very irregularly formed, occupy a still greater space. In both cases, however, the tourmaline and the quartz, when taken out, leave large openings in the laminæ, and have greatly disturbed the structure of the mica around them.

The crystals of tourmaline thus formed in the mica, have almost always the faces of the flattened hexagonal prism parallel to the laminæ of the mica. I have found, however, a few cases in which the flat summit of the hexagonal prism is parallel to the laminæ. The crystallisations of quartz have also the axis of the prism, or its hexagonal faces parallel to the laminæ.

The other crystals of tourmaline which I have discovered in mica have a very different character: They have been formed subsequently to the crystallisation of the mica, and exist only between its laminae. I have not been able to discover any cavities in mica containing fluids or gases, but I have found thousands from which the fluids and gases have escaped,—the one crystallising into hexagonal plates of tourmaline, and the other separating the laminae, or running between them, and carrying along with it minute portions of crystallisable matter.

The hexagonal crystals thus formed have their faces perpendicular to the axis of double refraction, which is the axis of the prism; and what is peculiarly interesting, the fluid from which they were formed has insinuated itself between several of the laminae, and the different plates of tourmaline which they formed have, of course, the sides of the hexagon incoincident. Sometimes these crystals extend to different distances from the centre of the original cavity, and are occasionally formed round it in a circular group. See Plate XV., Fig. 1.

The centre of the cavity from which these crystals have been projected is occupied by a spherical group of granular or capillary crystals, which is generally very *opaque*, though such groups sometimes exhibit, in particular spots, double refraction, and a speck of light is occasionally seen through the centre of the group. In some cases I have observed these very thin hexagonal plates without this opaque centre; and they have probably been formed by a portion of the fluid projected to a distance between faces of easy cleavage. The black spherical group already mentioned has its outward surface bristled with points, which are the extremities of the crystals radiating from its centre; and in one fine specimen to be farther described, it is surrounded with a ring of less opacity than the nucleus, and analogous to what is common in circular crystals. See Fig. 1.

The thin plates thus formed between the laminae, whether hexagonal or prismatic, are always of a faint *brownish yellow*, which at an increased thickness becomes *green*; and so exceedingly thin are these plates, especially those farthest from the nucleus, that with a power of 400, it is often very difficult to see their terminal lines.

In order to convey an idea of these phenomena, I have given a drawing in Fig. 1 of a very interesting one, where the prismatic crystal nearest the black central group is a bright *green* in all azimuths with polarised light, surrounded with three or four larger prismatic yellowish plates, growing fainter both in tint and outline as they recede. In some cases the crystals are brown, and in others beautifully dichroitic, being *bright green* and *pink* in the different azimuths of polarised light.

As considerable forces must have been in operation during the production of these phenomena, we may expect to see the effects of them upon the surrounding mica. We accordingly observe the polarisation produced by pressure round almost all of these crystalline groups. Rents and other marks of violence are dis-

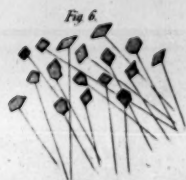
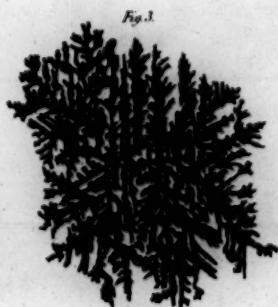
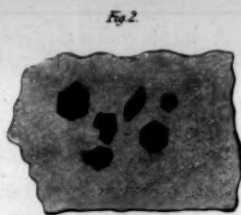
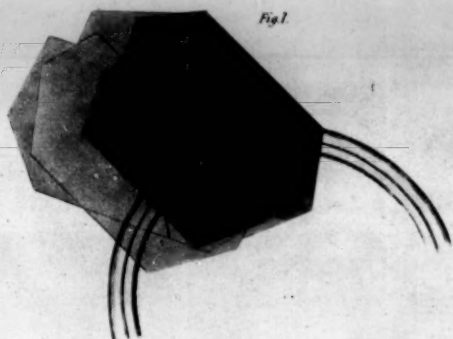


Fig. 7.



tinctly seen in the mica, and cracks or luminous streaks often occur in the tourmaline plates themselves. I have observed, too, in portions of the mica where I cannot find any cavities or crystals, distinct luminous sectors of polarised light, which could only be produced by a force emanating from their centre. This force may have been that of gas discharged from some neighbouring cavity, and driven by change of temperature to some other part of the mica plate; and in the following remarkable phenomenon we may perhaps find some evidence in favour of this opinion.

Plates of mica contain many beautiful systems of Newton's rings, occupying a circular space where the laminae have been separated by some cause or other, and where, of course, there must be either air or some gaseous body. The colours of the first order are at the circumference of the circular space where the laminae are in optical contact, and the higher orders of colour extend towards, and often to the centre of the space. Now it is a curious fact, that wherever there is a cavity which has projected its *fluid* and probably *gaseous* contents, *it is situated in the circumference of one of these circular spaces*. When two cavities have been near each other, the circular spaces unite and lose their form, and when the cavities have been more numerous, the circular spaces unite into very irregular shapes. That these circular hollows or spaces between the laminae have been produced by something which has issued from the cavity to which they are so constantly related, cannot admit of a doubt. That it has not been a fluid is evident, and therefore it must have been a gas, which is either there still, or has escaped through some minute openings between the laminae, where optical contact has been restored.*

There are some specimens of mica in which the crystals of tourmaline are large and opaque, and exhibit phenomena which I believe have not been recognised in any other mineral. The most interesting specimen of this kind I owe to Professor FLEMING, who pointed out to me one of the peculiarities which it contains. This specimen is accurately represented, of the natural size, in Fig. 2. The largest of the five crystals is 0.28 of an inch broad, and the smallest 0.08 of an inch. Their thickness cannot greatly exceed the thousandth of an inch, and yet it is with difficulty that the strongest sun-light can be seen through them. The form of the smallest is a perfect hexagon, and in the rest the same form is more or less distinct. In the oval crystal there are numerous holes, and in all of them there are numbers of rectilinear cracks parallel to the sides of the hexagon, and some of them so narrow that light can scarcely pass through them. When we look at the sun through one of these crystals, a curious optical phenomenon is seen, a luminous hexagonal surface, composed of lines of light, parallel to the

* A fluid even may have thus escaped, and the circular hollow remained as before. In support of this opinion, see *Edinburgh Transactions*, vol. x., p. 11; but especially vol. xvi., p. 13; or *Phil. Mag.*, vol. xxxi., p. 101, August 1847.

sides of the hexagon, and six beautiful radiations, like those of the Asterial Sapphire, perpendicular to the sides of the hexagon.

The existence of these rectilinear fissures is an important fact in crystallography. It proves that the crystals were in a soft state after they had attained their present form; and that, in the process of induration, the fissures were produced by the shrinking of the tourmaline, in the same manner as similar fissures are produced during the induration of clay. In the mica which surrounds some of the crystals, there is the appearance of considerable disturbance; but I can find no trace of any cavity from which the tourmaline may have been ejected in a fluid state. The faces of these crystals are not everywhere in optical contact with the mica, and it is very probable that they could be removed without any adhering mica, as I have occasionally found crystals of tourmaline that were moveable between the laminae.

In the same specimen which contains these tourmalines, and in others, I have found, what I believe has never before been observed, the woolly filaments of the *Penicillum glaucum* of LINK, with its sporules scattered about between the laminae, and sometimes beautifully monilliform, as in the *Penicillum glaucum* obtained from milk by M. TURPIN.*

2. On the Distribution of Titanium in Mica.

In examining a remarkable specimen of mica from Irkutsk, in Siberia, I found *titanium* between the laminae in various forms, sometimes in amorphous plates, sometimes in a powdery state adhering to the mica, and most frequently in beautiful dendritic forms of various degrees of thickness. At a thickness of about the hundredth of an inch, the titanium, in all these forms is opaque; but at less thicknesses, it has a brownish transparency, becoming almost perfectly transparent at thicknesses which do not seem to exceed the 2000th part of an inch. In Fig. 3 I have given a drawing of an opaque group executed for me with minute accuracy by my celebrated friend Mr HAIDINGER of Vienna, during his residence in Edinburgh. The transparent groups are much more beautiful than the opaque ones, the crystalline ramifications having the most diversified forms, resembling often regular organisations.

When the *mica* is removed from above the titanium, so that only an exceedingly thin film of it is left, the reflected light is extremely brilliant, and consists of the most splendid colours. These colours, which have always the form of the titanium, are those which are produced by the thin film of mica which covers the titanium, and are not produced, as has been supposed, by a vacuity in the mica.

In some specimens of mica from Bengal, the imbedded titanium is spread out

* See *Comptes Rendus*, tom. v., p. 822, 1837, Dec. 11.

in a very irregular manner from a nucleus, sometimes having the form of a thin film; sometimes of oriental characters; and sometimes it is disseminated in grains so extremely minute, that the flame of a candle seen through it is surrounded with a halo of five or six perfectly-formed coloured rings.

3. *Distribution of Quartz in Mica.*

In mica from various localities, I have found large crystallisations of quartz, the quartz replacing the mica. I have never even once met with a regular crystal of quartz; and what is curious, all the crystalline masses of it which I have examined have their axis of double refraction in the plane of the laminae of mica. In some very large specimens of Bengal mica given to me by Mr SWINTON, I have found layers of quartz, several inches in area, and about the 200th of an inch thick. The two surfaces of the plates are exceedingly unequal and corrugated, owing to the circumstances under which they were formed, but they possessed regular double refraction, and gave the colours of polarised light.

4. *Distribution of Titanium in Amethyst.*

While examining, many years ago, along with the late MARQUIS of NORTHAMPTON, several bags of amethyst which had been imported into Scotland from the Brazils, we were surprised to observe a number of fine pyramidal crystals, which seemed to have a powdery matter distributed through their mass. Upon more narrowly examining these crystals, I found that this dust formed an inner pyramid, all the faces of which were parallel to the faces of the pyramid of amethyst. When two parallel faces were ground upon the pyramid, and perpendicular to its axis, the particles of dust were seen by the microscope to consist each of several spicular crystals of titanium, crossing one another at angles of 60° and 30° , and forming distinct groups. In one crystal there were two interior pyramids composed of these groups; and it will be seen, from the explanation which I shall presently give of this phenomenon, that there may be any number of such pyramids.

As the crystals of amethyst are supposed to have been produced by the gradual enlargement of a small crystal placed in an amethystine solution, we have only to assume that a solution containing titanium has been introduced into the amethystine solution at different times during the growth of the crystal. The small crystals of titanium will deposit themselves on each of the surfaces of the pyramid; and when the whole of the introduced titanium has been thus deposited, the enlargement of the amethyst will go on, leaving a pyramid of titanium crystals in its interior. If a second solution of titanium is introduced, a second pyramid of its particles will be formed in the same manner; and this process may be repeated any number of times.

If we now suppose that the amethystine solution is exhausted, just when the

titanium solution has deposited all its crystals, the completed crystal of amethyst will have its outer surfaces covered with spicular crystals of titanium, or the pyramid of titanium groups will be on the very outside of the pyramid of amethyst. I had the good fortune to find such a crystal, in which the coat containing the titanium is laid like varnish on all the faces of the pyramid, but only on the upper end of three of them; the lower end of these three faces having lain on the solution protected from the deposition of the titanium. This crystal is, I believe, unique, and possesses the great interest of exhibiting the very process by which it was formed.

The two phenomena which I have just described are shewn in Figs. 4 and 5.

5. *Distribution of Titanium in Brazil Topaz.*

In examining a great number of very imperfect crystals of Brazil topaz, I found many which contained crystals of titanium of a brilliant scarlet colour, with a tinge of yellow. These crystals were perfectly transparent, and occurred in *seven* different forms.

1. In flat amorphous plates, which were highly transparent.
2. In hexagonal plates, lying in different planes.
3. In transparent lines running in different directions, and, though continuous, lying in different planes.
4. In lines running inwards from the margin of the specimen, and terminating in small flat plates. See Fig. 6.
5. In the most remarkable symmetrical forms like sceptres or maces, resembling some of those symmetrical cavities which I had previously found in the white topazes of New Holland.* See Fig. 7.
6. In some specimens the plates of titanium are actually bent, as in Fig. 8.
7. In little groups of transparent circular plates of a scarlet colour, and having concentric rings. -

When light is reflected from the separating faces of the titanium and topaz, it is almost completely polarised; and at greater angles than that of maximum polarisation, colours of singular brilliancy cross the reflected images. These colours are doubtless connected with the fact, that at some of these faces there are *three* images of a luminous object seen by reflexion, one of the two outer ones being polarised oppositely to one of the double middle images, as in the case of the multiplication of images in composite crystals of calcareous spar.†

6. *On the Crystals and Cavities in Garnet.*

In the greater number of the crystals of garnet which I have had occasion to examine, I have found many crystals and cavities, and much amorphous matter. In

* See *Edinburgh Transactions*, 1826, vol. x., Plate XX.

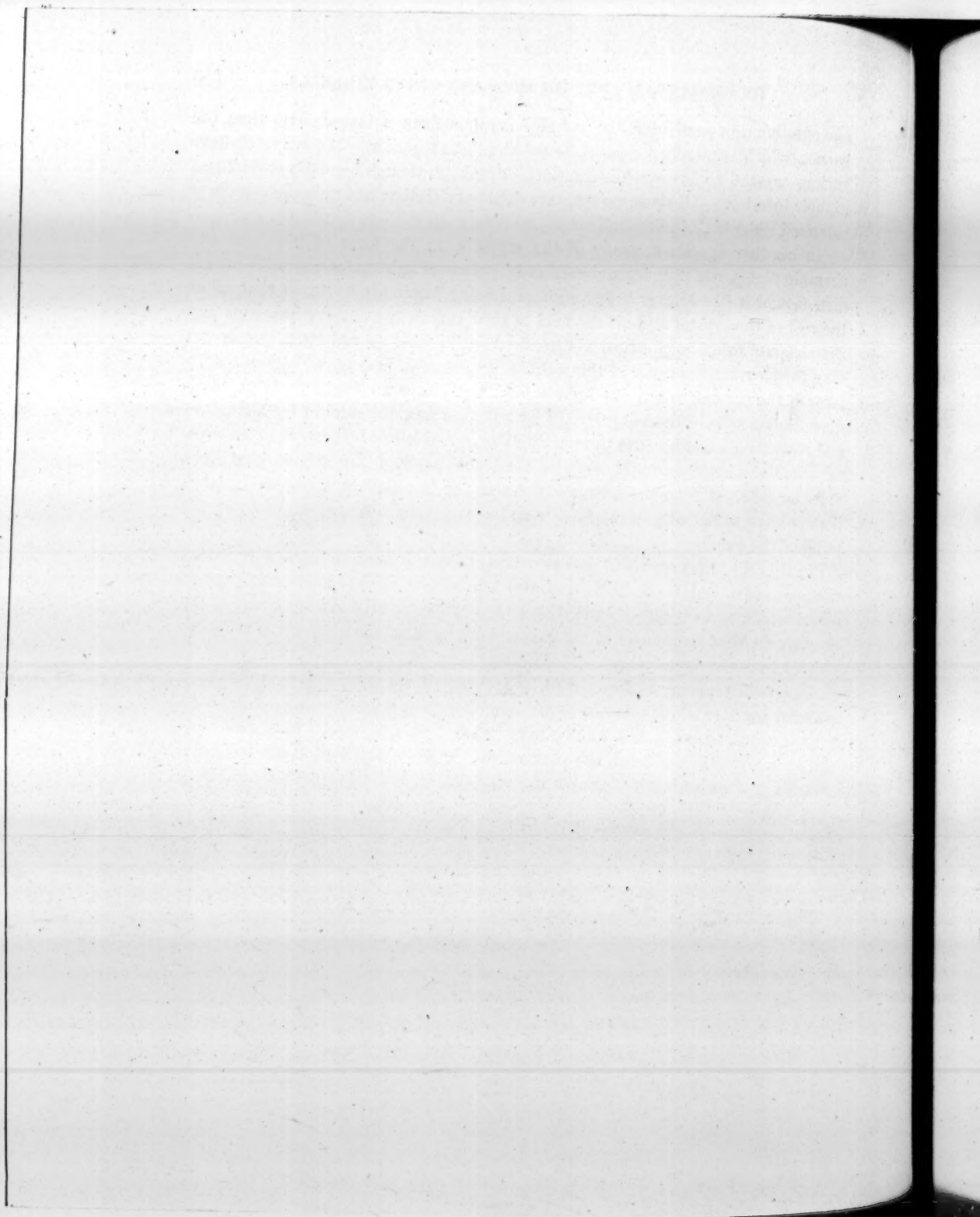
† See *Phil. Trans.*, 1815, Plate XV., Fig. 2.

one specimen, in particular, the included crystals form a larger mass than the garnet, which is merely a cement for holding them together. These crystals have various crystalline forms, while some are amorphous, though regularly crystallised in their interior. All these crystals are doubly refracting, and give the colours of polarised light from their small size.

In another specimen, many of the crystals, in the form of hexagons and rhombic plates, are opaque, and exhibit by polarised light the remarkable phenomenon, which I had never before seen, of having luminous edges, so that when the rest of the crystal and all the field of view is dark, we observe hexagons and rhombs, and other geometrical figures, depicted in lines of red light. It is not easy to ascertain the cause of this singular appearance, because we cannot see the form of the crystals where the light exists; but I have no doubt that the luminous lines consist of light depolarised by reflexion from the sides of the hexagonal and rhombic plates, because the illuminating pencil is much larger than the crystals, and the crystals much smaller than the pupil of the eye, so that light must be reflected from the prismatic faces of the hexagons and rhombic plates, if they have sufficiently broad faces, and that light so reflected must enter the pupil of the eye.

In this specimen and in others there are many spherical cavities, surrounded with sectors of polarised light, and also several amorphous masses of matter, round which there is also polarised light, indicating, as all the phenomena of the crystals do, that the matter of the garnet must have been in a soft state, and compressed by some force emanating from these cavities.

In another specimen of garnet, a large fissure in its interior is occupied with granular matter, which must have issued either from a burst cavity containing a fluid or a gas, or both; but what is very interesting, and what I have never observed in any other mineral, the matter has, in several places, formed circular crystals of singular beauty, some being very simple, and others very composite.



XXXVIII.—*On the Production of Crystalline Structure in Crystallised Powders, by Compression and Traction.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., V.P.R.S. Edin., and Associate of the Institute of France.

(Read 7th March 1853.)

The influence of compression and dilatation in producing the doubly refracting structure in solids of all kinds, whether crystallised or uncrystallised, which do not possess it, and in modifying that structure in all crystals which do possess it, has been long known; but with this class of phenomena, those which I am about to describe have no connection whatever.

In the course of experiments on the *double reflexion* and polarisation of light which I discovered in the *chrysammates of potash and magnesia, murexide*, and other crystals, I was surprised to find that these substances could be spread out upon glass by hard pressure, like grease or soft wax, and that in the case of chrysammate of potash and other bodies, when the powder could scarcely be distinguished from snuff, I obtained a transparent film, exhibiting the phenomena of double reflexion and polarisation from its surface, as perfectly as if I had been using a large crystal.

In subsequently repeating these experiments, and examining, under polarised light, the film thus produced by compression and traction, I was surprised to observe that the streaks and separate lines of the film, as well as the film itself, had regular axes of double refraction, as if they were regularly crystallised portions of the substance under examination. These streaks and capillary lines, which were often of extreme minuteness, did not appear to consist of insulated particles merely dragged into a line, but when the substance possessed the new property in perfection, the lines of polarised light were continuous, and the crystallographic as well as the optical axes of the particles were placed in that line. In other cases, where the experiment was less successful, the insulation of the particles was easily recognised, though the general mass of them was crystallographically arranged.

In making these experiments, the natural crystalline powder, or the particles of the crushed crystal, may be placed, either upon a polished glass surface, or upon a piece of glass ground on one side. In those cases where the substance is soft, the polished surface is preferable, but when the powder is hard and considerable pressure necessary, it is better to place it upon the ground surface of a piece of glass,

as the particles are detained between its minute elevations, and submit more readily to the combined force of pressure and traction. When the powder is thus placed, I take a polished and elastic knife, and with its broad point I compress and drag the powder in a given direction, till there is the appearance of a polished surface on the compressed substance. In general, I have used both the smooth and the rough glass, and have frequently obtained results with the one, which were not given by the other.

If we now place the plate of glass in a polarising microscope, with the field dark, we shall find that the streaks and lines produced by traction have, in certain substances, regular neutral and depolarising axes, as if they were prismatic crystals of the substance under examination. With the *chrysammate of magnesia*, a red powder with specks of yellow reflected light, the phenomena are peculiarly splendid; the natural colours of the substance, which vary greatly with the thickness of the streaks and films, being combined with the different tints which they polarise. As the crystals of this substance possess unusual reflexion, this property is displayed in the crystallised streaks produced by traction; and the superficial colours which they reflect, vary with the azimuth which the plane of incidence forms with the plane passing through the axis of the prism.

The remarkable property which I have now described, I have found, in a greater or a less degree, in the following crystals:—

Chrysammate of magnesia.

... of potash.

Hydro-chrysamid.

Murexide.

Aloetinate of potash.

Aloetic acid.

Oxamide.

Palmine.

Palmic acid.

Amygdaline.

Tannin, pure.

Quinine, pure.

... acetate of.

... sulphate of.

... muriate of.

... phosphate of.

... citrate of.

Cacao butter.

Veratric acid.

Esculine.

Theine.

Silver, cyanide of.

... acetate of.

Platina and magnesia, cyanuret of.

... and barytes, cyanuret of.

... potassium, cyanuret of.

... ammonia, chloride of.

Potash, oxymuriate of.

... chromate of.

Urea, nitrate of.

Sulphur.

Camphor.

Cinchonine.

... sulphate of.

Meconic acid.

Brucine, sulphate of.

Morphia, acetate of.

Tin, iodide of.

Cerium, oxide of.

Parmeline.

Lecanorine.

Indigo, red.

Ammonia, oxalate of.

... sulphate of.

Soda, chromate of.

Lead, iodide of.

Strychnine, sulphate of.
 ... acetate of.
 Soda, native nitrate of.
 Berberine.
 Mucic acid.
 Solanine.
 Asparagine.

Mercury, oxymuriate of.
 Isatine.
 Alizarine.
 Manganese, sesquioxide of.
 Lead, protoxide of.
 Tungstic acid.
 Chromo-oxalate of potash.

In submitting other crystals to the influence of compression and traction, I have found great numbers which do not exhibit the least trace of transparent streaks and lines, the separate particles being merely dragged into lines, and exhibiting only a quaquaversus polarisation. On the other hand, there is another class of crystals, whose powders or particles are forced into distinct and transparent streaks and lines in which the individual particles have a quaquaversus polarisation, and no trace of a prismatic arrangement. As these crystals have a peculiar relation to those in the preceding list, I shall enumerate the most important of them in the following table; that is, those in which the powder has been dragged into transparent and continuous streaks and lines, resembling externally portions of a solid body; for it is only by a comparison of the physical, or perhaps the chemical qualities of the two classes of bodies, that we can expect to explain the new property which is possessed only by one of them.

Hydrate of potash, pure.
 Indigotic acid.
 Urea.
 Citric acid.
 Silver, nitrate of.
 Meconine.
 Naphthaline.
 Soda, nitrate of.
 Potash and copper, sulphate of.
 Soda, phosphate of.

Soda, acetate of.
 Mercury, prussiate of.
 ... muriate of.
 ... sulphuret of.
 Barytes, acetate of.
 Zinc, chromate of.
 ... sulphate of.
 Cobalt, sulphate of.
 Magnesia and soda, sulphate of.
 Borax.

As both compression and traction are necessary in producing the transparent streaks and lines in both classes of the substances I have enumerated, it became interesting to ascertain what effect was produced by each of these forces acting separately, and which of them was chiefly influential in developing the doubly refracting arrangement exhibited by the substances that possessed it.

The force of compression was undoubtedly the agent in forcing the separate particles into optical contact, while that of traction drew them into a line, and tended to dilate the film in the direction of that line, and to draw its particles from each other; or overcome their attraction of aggregation in that direction. It is quite possible, too, that these forces may have exercised some influence in modifying the doubly refracting structure of the substance under examination; but as such a question has no bearing upon our present subject, I have not attempted its solution.

Without expecting any very interesting result, I submitted to examination several of the soft solids which possess double refraction, such as *bees' wax*, *oil of mace*, *tallow*, and *almond soap*. The last of these substances, though in common use, is a very remarkable one. Owing to its particles not being in optical contact, it has a fine pearly lustre, and may be drawn out into long and slender strings. Upon laying a portion of it on glass, it has a *quaquaversus* polarising structure, with a tendency to form circular crystals, but when it is drawn out into strings, and laid upon glass, these strings have neutral and depolarising axes, like the streaks formed by compression and traction. In the present case, it is by traction alone, that this crystalline arrangement of the particles is produced.

In *oil of mace* and *tallow*, a similar effect is produced by compression and traction. With *bees' wax*, the depolarising lines are still better displayed, and the effect is considerably increased by mixing the bees' wax with a small quantity of rosin.

As the preceding experiments place it beyond a doubt, that the optical or crystallographic axes of a number of minute particles are dragged by pressure and traction into the same direction, so as to act upon light like regular crystals, it became interesting to discover the cause of phenomena which certainly could not have been anticipated from any theoretical principle with which we are acquainted. The primary force, and indeed the only apparent one exerted in these experiments, is a mechanical force; but it is not improbable that a secondary force, namely, that of electricity, may be generated by the friction which accompanies the forces of pressure and traction. That such a force is excited with certain crystals will not admit of a doubt; but even if it were developed in every case, this would not prove that electricity was the agent in producing the phenomena under consideration. In subjecting asparagine to compression and traction, I observed, upon placing it in the polarising microscope, that its particles were moving about under an electrical influence, but in no other case did the same phenomenon present itself to me.

The experiments with soft solids, but especially those made with the almond soap, exclude the supposition that the electricity of friction is the cause of the crystalline arrangement of its particles; though it is not improbable that the sliding of the particles upon one another, as produced by traction, and their mutual separation, as in the case of tearing asunder mica or paper, may produce enough of electricity to have some share in giving the same direction to the axes of the particles.

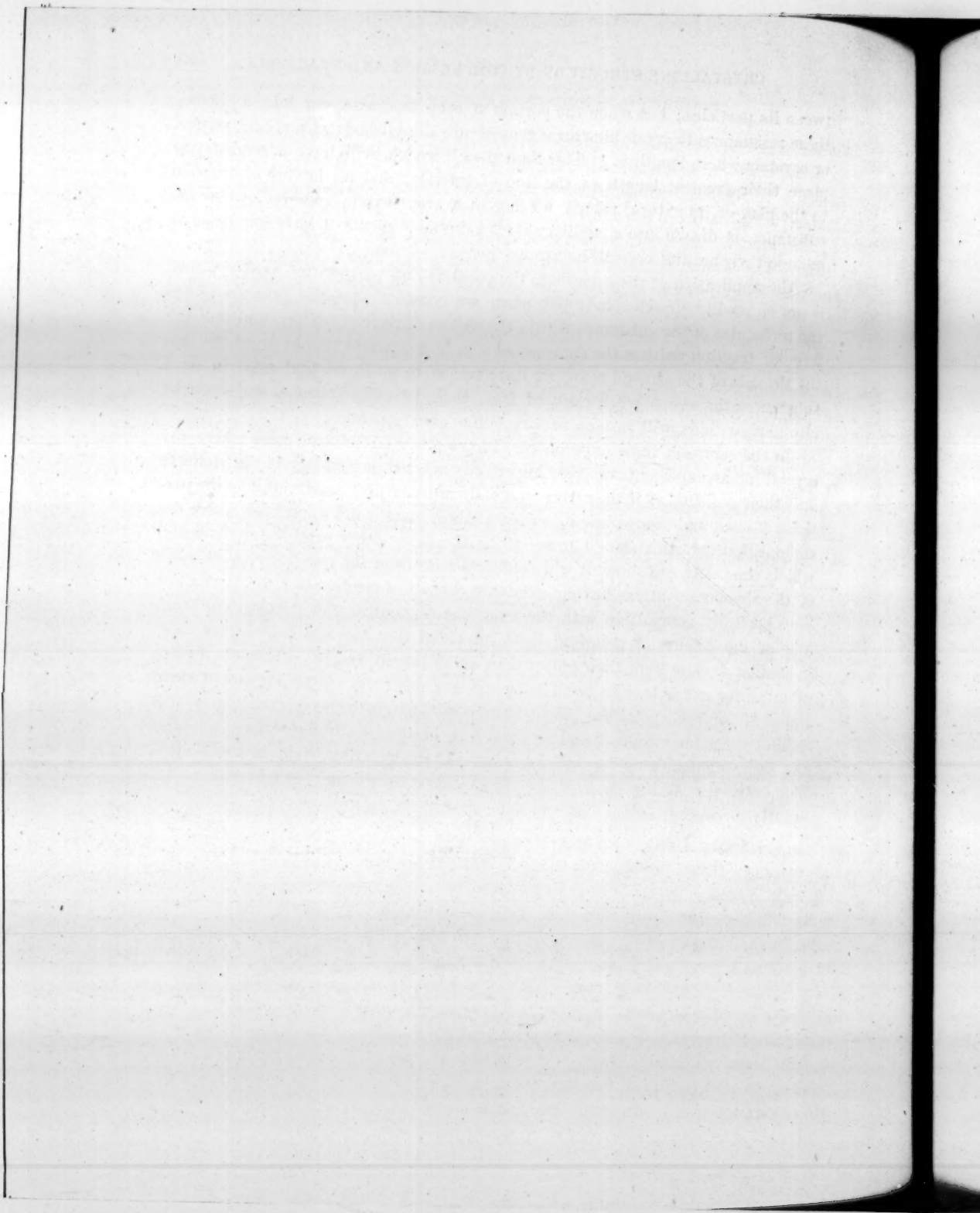
When a portion of almond soap is placed upon glass, the axes of its particles lie in every direction, and have no tendency to assume the crystalline arrangement. The forces of aggregation emanating from three rectangular axes, are not strong enough to overcome the inertia, as we may call it, arising from the natural *quaquaversus* adhesiveness of the substance, and from the water interposed be-

tween its particles; but when the portion of soap is drawn out into a thread, these resistances to crystalline arrangement are diminished; elementary prisms, or crystals whose length is greater than their breadth, will have a tendency to place their greatest length in the line of traction, and all lateral obstruction to the play of its natural polarities being to a great extent removed, when the substance is drawn into a capillary thread the molecules will have free scope to assume their natural crystalline arrangement.

The application of these views to the powders and particles of hard crystals, is not so readily apprehended; but when we consider that the pressure brings the molecules of the substance within the sphere of their polarities, and that the force of traction reduces the compressed film into separate streaks and lines, like the threads of the almond soap, we have reason to conclude, that even in hard substances the atoms, when released from their lateral adhesions, and brought into narrow lines, will assume the crystalline arrangement.

In the course of these experiments, I have observed, in some cases where the crystalline arrangement was very imperfectly effected, a tendency in the atoms to quit their position, as if they were in a state of unnatural constraint, like the particles of silex and manganese in certain kinds of glass which experience a slow decomposition. If this should prove to be the case, either partially or generally, which time only can shew, it will doubtless arise from the non-homologous sides of the elementary atoms having come into contact, a condition of the crystalline lines perfectly compatible with the existence of neutral and depolarising axes, and of the colours of polarised light, provided that the non-homologous sides in contact deviate from their proper position, either 90° or 180° . If we cut a plate of mica, for example, into two pieces, and combine them by turning one of them round 90° or 180° , polarised light transmitted through them perpendicularly, will exhibit the same colours as when they were in their natural position, and also the same neutral and depolarising axes. If the polarised light is transmitted obliquely, the *hemitropism* of the combination, as we may call it, will be instantly discovered by the difference of colour of the two plates.

ST LEONARD'S COLLEGE, ST ANDREWS,
February 25, 1853.



XXXIX.—*On the Absolute Zero of the Perfect Gas Thermometer; being a Note to a Paper on the Mechanical Action of Heat.* By WILLIAM JOHN MACQUORN RANKINE, C.E., F.R.S.E., F.R.S.S.A., &c.

(Read January 4, 1853.)

Temperature being measured by the pressure of a perfect gas at constant density, the absolute zero of temperature is that point on the thermometric scale at which, if it were possible to maintain a perfect gas at so low a temperature, the pressure would be null.

The position of this point is of great importance, both theoretically and practically; for by reckoning temperatures from it, the laws of phenomena depending on heat are reduced to a more simple form than they are when any other zero is adopted.

As we cannot obtain any substance in the perfectly gaseous condition (that is to say, entirely devoid of cohesion), we cannot determine the position of the absolute thermometric zero by direct experiment, which furnishes us with approximate positions only. Those approximate positions are always too high; because the effect of cohesion is to make the pressure of a gas diminish more rapidly with a diminution of temperature, than if it were devoid of cohesion.

As a gas is rarefied, the cohesion of its particles diminishes, not only in absolute amount, but also in the proportion which it bears to the pressure due to heat. The gas, therefore, approaches more and more nearly to the state of a perfect gas as its density diminishes; and from a series of experiments on the rate of increase of its elasticity with temperature, at progressively diminishing densities, may be calculated the positions of a series of points on the thermometric scale, approaching more and more nearly to the true absolute zero.

By observing the law which those successive approximations follow, the true position of the absolute zero can be determined.

Having performed this operation by means of a graphic process, soon after the publication of the experiments of M. REGNAULT on the elasticity and expansion of gases, I stated the result in a paper on the Elasticity of Vapours (*Edinburgh New Philosophical Journal*, July 1849), and also in a paper on the Mechanical Action of Heat (*Trans. Royal Soc. Edin.* vol. xx., Part 1), viz., that the absolute zero is

274.6 centigrade degrees,	} below the temperature of melting ice;
or 494.28 degrees of FAHRENHEIT,	
or 462.28 degrees below the ordinary zero of FAHRENHEIT'S scale.	

To enable others to judge of the accuracy of this result, I shall now explain the method by which it was obtained, annexing a copy of the diagram used.

Let E denote the mean rate of increase, per degree, between the freezing and boiling points, of the pressure of a gas whose volume is maintained constant. Then the reciprocal of this coefficient, $\frac{1}{E}$, is an approximation to the number of degrees below the freezing point, at which the absolute zero is situated.

The experimental data in the following table were copied from the memoirs of M. REGNAULT on the Expansion of Gases. The numbers in the first column designate the series of experiments. The second column contains the pressures of the gases at the freezing point. The third column contains the mean coefficients of increase of pressure per centigrade degree, between 0° and 100° centigrade. The fourth column contains the reciprocals of those coefficients, with the negative sign, being approximate positions of the absolute zero, in centigrade degrees, below the temperature of melting ice. The gases employed were atmospheric air and carbonic acid.

No.	Pressure at 0° Centigrade in Atmospheres.	Coefficient of in- crease of Elasticity with Temperature = E .	Approximate positions of the absolute zero in Centigrade degrees = $\frac{1}{E}$
CARBONIC ACID.			
1.	0.9980	0.0036856	-271.33
2.	1.1857	0.0036943	-270.63
3.	2.2931	0.0037523	-266.50
4.	4.7225	0.0038598	-259.08
ATMOSPHERIC AIR.			
1.	0.1444	0.0036482	-274.11
2.	0.2294	0.0036513	-273.88
3.	0.3501	0.0036542	-273.66
4.	0.4930	0.0036587	-273.32
5.	0.4937	0.0036572	-273.43
6.	1.0000	0.0036650	-272.85
7.	2.2084	0.0036760	-272.03
8.	2.2270	0.0036800	-271.74
9.	2.8213	0.0036894	-271.05
10.	4.8100	0.0037091	-269.61

The approximate positions of the absolute zero contained in this table were laid down on the diagram, in which they are marked by crosses. The longitudinal divisions represent centigrade degrees divided into tenths; the transverse divisions, atmospheres of pressure at 0° centigrade, also divided into tenths. The positions of the crosses indicate at once the pressures in the second column of the table, and the approximate zeros in the fourth; and the numbers affixed to them correspond with those in the first column.

As the effect of cohesion is greater, and more easily eliminated, in carbonic acid gas than in atmospheric air, the determination of the true absolute zero was made from the experiments on the former gas. It will be observed that the approximate positions of the absolute zero for carbonic acid lie nearly in a straight line. A straight line (dotted in the diagram) having been drawn so that it should as nearly as possible traverse them, was found to intersect the line corresponding to the zero of pressure, that is, to the state of perfect gas, at a point on the scale of temperatures 274·6 centigrade degrees below the temperature of melting ice; which point was accordingly taken as the true absolute zero of the perfect gas thermometer.

So far as their irregularity permits, the experiments on atmospheric air confirm this result, for the approximate positions of the absolute zero deduced from them, evidently tend towards the very same point on the diagram with those deduced from the experiments on carbonic acid.

The values of the coefficient of dilatation and of increase of pressure, of a perfect gas, per degree, in fractions of its volume and pressure, at the temperature of melting ice, are accordingly,—

$$\text{For the Centigrade Scale } \frac{1}{274\cdot6} = 0\cdot00364166$$

$$\text{For FAHRENHEIT's Scale } \frac{1}{494\cdot28} = 0\cdot00202314$$

THE HISTORY OF THE UNITED STATES

The history of the United States is a story of growth and development. It begins with the first settlers who came to the continent in search of a new home. These settlers found a land of vast resources and potential, but they also found a land that was already inhabited by a diverse and complex society of Native Americans. The story of the United States is a story of the struggle to create a new society, a society that would be based on the principles of liberty and justice for all. It is a story of the challenges that have been faced, the triumphs that have been achieved, and the lessons that have been learned. The history of the United States is a story that is still being written, and it is a story that is important to all of us.

XL.—*On the Mechanical Action of Heat.* By WILLIAM JOHN MACQUORN RANKINE,
Civil Engineer, F.R.S.E., F.R.S.S.A., &c.

(Read January 17, 1853.)

SECTION VI.—A REVIEW OF THE FUNDAMENTAL PRINCIPLES OF THE MECHANICAL
THEORY OF HEAT; WITH REMARKS ON THE THERMIC PHENOMENA OF CURRENTS
OF ELASTIC FLUIDS, AS ILLUSTRATING THOSE PRINCIPLES.

(Article 46.) I have been induced to write this Section, in continuation of a paper on the Mechanical Action of Heat, by the publication (in the *Philosophical Magazine* for December 1852, Supplementary Number) of a series of experiments by Mr JOULE and Professor WILLIAM THOMSON, on the Thermal Effects experienced by Air in rushing through small Apertures. Although those authors express an intention to continue the experiments on a large scale, so as to obtain more precise results; yet the results already obtained are sufficient to constitute the first step towards the experimental determination of that most important function in the theory of the mechanical action of heat, which has received the name of CARNOT's Function.

By the theoretical investigations of Messrs CLAUSIUS and THOMSON,—which are based simply on the fact of the convertibility of heat and mechanical power, the determination of their relative value by Mr JOULE, and the properties of the function called *temperature*, without any definite supposition as to the nature of heat,—CARNOT's function is left wholly indeterminate.

By the investigations contained in the previous sections of this paper, and in a paper on the Centrifugal Theory of Elasticity,—in which the supposition is made, that heat consists in the revolutions of what are called Molecular Vortices, so that the elasticity arising from heat is in fact centrifugal force,—a *form* is assigned to CARNOT's function; but its numerical values are left to be ascertained by experiment.

The recent experiments of Messrs JOULE and THOMSON serve (so far as the degree of precision of their results permits) at once to determine numerical values of CARNOT's function for use in practice, and to test the accuracy with which the phenomena of heat are represented by the consequences of the hypothesis of molecular vortices, from which the investigation in this paper sets out.

SUB-SECTION 1.—*Properties of Expansive Heat.*

(47.) To shew more clearly the nature of the questions, towards the decision of which these experiments are a step, I shall now briefly review the fundamental principles of the theory of heat, and the reasoning on which they are based; and the object of this being illustration rather than research, I shall use algebraical symbols no farther than is absolutely necessary to brevity and clearness, and shall follow an order of investigation, which, though the same in its results with that pursued in the previous sections of this paper, is different in arrangement.

By a mind which admits as an axiom, that, in the present order of things, physical power cannot be annihilated, nor produced out of nothing, the law of the mutual convertibility of heat and motive power must be viewed as a necessary corollary from this axiom, and Mr JOULE's experiments, as the means of determining the relative numerical value of those two forms of power. By a mind which does not admit the necessity of the axiom, these experiments must be viewed also as the proof of the law.

This law was virtually, though not expressly, admitted by those who introduced the term Latent Heat into scientific language; for when divested of ideas connected with the hypothesis of a subtle fluid of caloric, and regarded simply as the expression of a fact, this term denotes heat which has disappeared during the appearance of expansive power in a mass of matter, and which may be made to reappear by the expenditure of an equal amount of compressive power.

(48.) Without for the present framing any mechanical hypothesis as to the nature of heat, let us conceive that unity of weight of any substance, occupying the bulk V under the pressure P , and possessing the absolute quantity of thermometric heat whose mechanical equivalent is Q , undergoes the indefinitely small increase of volume dV ; and let us investigate how much heat becomes latent, or is converted into expansive power, during this process; the thermometric heat being maintained constant, so that the heat which disappears must be supplied from some external source.

During the expansion dV , the body, by its elastic pressure P , exerts the mechanical power $P dV$. Part of this power is produced by molecular attractions and repulsions; and although this part may be modified by the influence of heat upon the distribution of the particles of the body, it is not the direct effect of heat. The remainder must be considered as directly caused by the heat possessed by the body, of which the pressure P is a function; and to this portion of the power developed, the heat which disappears during the expansion must be equivalent.

To determine the portion of the mechanical power $P dV$ which is the effect of heat, let the total heat of the body, Q , be now supposed to vary by an indefinitely

increase of total heat dQ , while the volume remains constant, is expressed as follows:—

$$\frac{dS}{dQ} dQ = \left\{ Q \int \frac{d^2 P}{dQ^2} dV + \phi'(Q) \right\} dQ \quad (71.)$$

the heat Q being treated as a constant in the integration.

If we now investigate the entire quantity of heat, both sensible and latent, which is consumed by a body during a simultaneous small change of total heat dQ and volume dV , we find the following results:—

Sensible heat (which retains its condition) = dQ

Latent heat, or heat which disappears in overcoming
molecular action $\frac{dS}{dQ} dQ + \frac{dS}{dV} dV$

Latent heat equivalent to the visible mechanical effect $P dV$

The amount being

$$\left. \begin{aligned} dQ + dS + P dV &= \left(1 + \frac{dS}{dQ}\right) dQ + \left(\frac{dS}{dV} + P\right) dV = \\ &\left(1 + Q \int \frac{d^2 P}{dQ^2} dV + \phi'(Q)\right) dQ + Q \cdot \frac{dP}{dQ} \cdot dV \end{aligned} \right\} \quad (72.)$$

This formula expresses completely the relations between heat, molecular action, and expansion, in all those cases in which the expansive power developed, $P dV$, is entirely communicated to the bodies enclosing the substance which expands.

(49.) The following coefficients are contained in, or deducible from it.

The ratio of the specific heat at constant volume to the real specific heat:—

$$\frac{K_v}{K} = 1 + \frac{dS}{dQ} = 1 + Q \int \frac{d^2 P}{dQ^2} \cdot dV + \phi'(Q) \quad (73.)$$

The coefficient of latent heat of expansion at constant heat:—

$$\frac{dS}{dV} + P = Q \frac{dP}{dQ} \quad (74.)$$

The ratio of the specific heat at constant pressure to the real specific heat is found as follows. To have the pressure constant, we must have

$$\frac{dP}{dQ} dQ + \frac{dP}{dV} dV = 0; \text{ or } \frac{dV}{dQ} = - \frac{\frac{dP}{dQ}}{\frac{dP}{dV}}$$

consequently the ratio in question is

$$\left. \begin{aligned} \frac{K_p}{K} &= 1 + \frac{dS}{dQ} - \left(\frac{dS}{dV} + P\right) \frac{\frac{dQ}{dP}}{\frac{dV}{dP}} = 1 + Q \int \frac{d^2 P}{dQ^2} \cdot dV + \phi'(Q) \\ &\quad - Q \frac{\left(\frac{dP}{dQ}\right)^2}{\frac{dP}{dV}} \end{aligned} \right\} \quad (75.)$$

(50.) In order to investigate the laws according to which heat is converted into mechanical power, in a machine working by the expansion of an elastic body, it will be convenient to use a function

$$F = \int \frac{dP}{dQ} dV \quad (Q = \text{const.})$$

of such a nature that the difference between two of its values, corresponding to different volumes of the body at the same total heat, represents the ratio of the heat converted into power by expansion between those volumes, to the given constant total heat. I shall call this function a *heat-potential*.

Introducing this function into Equation 72, we find, for the total heat consumed by a body during the increase of total heat dQ , and the expansion dV ,

$$dQ + d.S + P dV = (1 + \phi' \cdot Q) dQ + Q d.F \quad (76.)$$

$$\left(\text{observing that } d.F = \frac{dF}{dQ} dQ + \frac{dF}{dV} dV = \left(\int \frac{d^2P}{dQ^2} dV \right) dQ + \frac{dP}{dQ} dV \right)$$

Let us now suppose that the body changes its volume without either losing or gaining heat by conduction. This condition is expressed by the equation

$$0 = (1 + \phi' \cdot Q) dQ + Q d.F$$

from which we deduce the following,

$$-d.F = \frac{1 + \phi' \cdot Q}{Q} \cdot dQ \quad (77.)$$

which expresses the following theorem:—

When the quantity of heat in a body is varied by variation of volume only, the variation of the heat-potential depends on the heat only, and is independent of the volume.

In order that a machine working by the expansive power of heat may produce its greatest effect, all the heat communicated from external bodies should be employed in producing expansive power, and none in producing variations of the quantity of heat in the body; for heat employed for the latter purpose would be wasted, so far as the production of visible motion is concerned. To effect this, the body must receive heat by conduction, and convert it into expansive power, while containing a certain constant quantity of heat Q_1 ; give out by conduction heat produced by compression, while containing a smaller constant quantity of heat Q_2 ; and change between those two quantities of thermometric heat by means of changes of volume only, without conduction. For this purpose a cycle of operations must be performed similar to that described by CARNOT; as follows:—

(I.) Let F_A be the initial value of the heat-potential; let the body expand at the constant heat Q_1 , till the heat-potential becomes F_B . Then the heat received and converted into expansive power is

$$H_1 = Q_1 (F_B - F_A)$$

(II.) Let the body further expand without receiving or emitting heat, till the quantity of heat in it falls to Q_2 ; the heat-potential varying according to Equation 77, and becoming at length F_c . The heat converted into expansive power in this operation is

$$Q_1 - Q_2$$

(III.) Let the body be compressed, at the constant heat Q_2 , till the heat-potential becomes F_b ; a quantity differing from the initial heat-potential F_a by as much as F_c differs from F_b . In this operation the following amount of power is reconverted into heat, and given out by conduction:—

$$H_2 = Q_2 (F_c - F_b)$$

(IV.) Let the body be further compressed, till the heat-potential returns to F_a , its original value. Then, by the power expended in this compression alone, without the aid of conduction, the total heat of the body will be restored to its original amount, exactly reversing the operation II.

At the end of this cycle of operations, the following quantity of heat will have been converted into mechanical power:—

$$H_1 - H_2 = Q_1 (F_b - F_a) - Q_2 (F_c - F_b)$$

but it is obvious that the difference between the heat-potentials is the same in the first and third operations; therefore, the useful effect is simply

$$H_1 - H_2 = (Q_1 - Q_2) (F_b - F_a) \quad \dots \dots \dots$$

while the whole heat expended is,

$$H_1 = Q_1 (F_b - F_a) \quad \dots \dots \dots \quad (78.)$$

Hence, *the ratio of the heat converted into mechanical effect, in an expansive machine working to the greatest advantage, to the whole heat expended, is the same with that which the difference between the quantities of heat possessed by the expansive body during the operations of receiving and emitting heat, respectively, bears to the quantity of heat possessed by it during the operation of receiving heat; and is independent of the nature and condition of the body.*

This theorem is thus expressed symbolically,—

$$\frac{H_1 - H_2}{H_1} = \frac{\text{Effect}}{\text{Heat Expended}} = \frac{Q_1 - Q_2}{Q_1} \quad \dots \dots \dots (79.)$$

(51.) When a body expands without meeting with resistance, so that all its expansive power is expended in giving velocity to its own particles, and when that velocity is ultimately extinguished by friction, then a quantity of heat equivalent to the expansive power is reproduced.

The heat consumed is expressed by taking away the term representing the expansive power, $P dV$, from the expression 72, the remainder of which consists merely of the variation of actual heat, and the heat expended in overcoming molecular attraction, viz. :—

$$dQ + d.S = \left(1 + \frac{dS}{dQ}\right) dQ + \frac{dS}{dV} dV = \left(1 + Q \int \frac{d^2 P}{dQ^2} dV + \phi'.(Q)\right) dQ \\ + \left(Q \frac{dP}{dQ} - P\right) dV.$$

This expression is a complete differential, and may be written thus:—

$$d(Q + S) = d \left\{ Q + \phi(Q) + \left(Q \frac{dP}{dQ} - 1\right) \int P dV \right\} \quad (80.)$$

(Q being treated as a constant in performing the integration $\int P dV$).

Its integral, $Q + S$, the sum of the heat of the body, and of the potential of its molecular actions, is the same quantity which I have denoted by the symbol Ψ in the 10th article of a paper on the Centrifugal Theory of Elasticity, and whose differences are there stated to represent the total amount of power which must be exercised on a body, whether in the form of expansive or compressive power, or in that of heat, to make it pass from one volume and temperature to another. This integral corresponds also to the function treated of by Professor WILLIAM THOMSON in the fifth part of his paper on the Dynamical Theory of Heat, under the name of "Total Mechanical Energy."

(52.) We have now obtained a system of formulæ, expressing all the relations between heat and expansive power, analogous to those deduced from a consideration of the properties of temperature, by Messrs CLAUSIUS and THOMSON, and from the Hypothesis of Molecular Vortices in the previous sections of this paper; but, in the present section, both the theorems and the investigations are distinguished from former researches by this circumstance;—that they are independent, not only of any hypothesis respecting the constitution of matter, but of the properties, and even of the existence, of such a function as Temperature; being, in fact, simply the necessary consequences of the following

DEFINITION OF EXPANSIVE HEAT.

Let the term EXPANSIVE HEAT be used to denote a kind of Physical Energy convertible with, and measurable by, equivalent quantities of Mechanical Power, and augmenting the Expansive Elasticity of matter, in which it is present.

(52 A.) It is further to be remarked, that the theorems and formulæ in the preceding articles of this section are applicable, not only to heat and expansive power, but to any two directly convertible forms of physical energy, one of which is actual, and the other potential. They are, in fact, the principles of the conversion of energy in the abstract, when interpreted according to the following definitions of the symbols.

Let Q denote the quantity of a form of actual physical energy present in a given body;

V, a measurable state, condition, or mode of existence of the body, whose tendency to increase is represented by

P, a force, depending on the condition V, the energy Q, and permanent properties of the body; so that

$P dV$ is the increment of a form of potential energy, corresponding to a small increment dV of the condition V.

Let dS be the quantity whereby the increment of potential energy $P dV$ falls short of the quantity of actual energy of the form Q, which is converted into the potential form, by the change of condition dV .

Then, as in Equation 69

$$\frac{dS}{dV} = Q \frac{dP}{dQ} - P$$

an equation from which all those in the previous articles are deducible, and which comprehends the whole theory of the mutual conversion of the actual form of energy Q, and the potential form $\int P dV$, whatsoever those forms may be, when no other form of energy interferes. The application of these principles to any form or any number of forms of actual and potential energy, is the subject of a paper read to the Philosophical Society of Glasgow, on the 5th January 1853, and published in the Philosophical Magazine for February 1853.

SUB-SECTION 2.—*Properties of Temperature.*

(53.) Still abstaining from the assumption of any mechanical hypothesis, let us proceed a step beyond the investigation of the foregoing articles, and introduce the consideration of *temperature*; that is to say, of an arbitrary function increasing with heat, and having the following properties.

Definition of Equal Temperatures.

Two portions of matter are said to have equal temperatures, when neither tends to communicate heat to the other.

Corollary.

All bodies absolutely destitute of heat have equal temperatures.

The ratio of the real specific heats of two substances, is that of the quantities of heat which equal weights of them possess at the same temperature.

Theorem.

The ratio of the real specific heats of any pair of substances, is the same at all temperatures.

For, suppose equal weights of a pair of homogeneous substances to be in contact, containing heat in such proportions as to be in equilibrio. Then, let additional

portions of each substance, of equal weight, and destitute of heat, be added to the original masses; so that the quantities of heat in unity of weight may be diminished in each substance, but may continue to be in the same ratio. Then, if the equality of temperature do not continue, portions of heat which were in equilibrium must have lost that equilibrium, merely by being transferred to other particles of a pair of homogeneous substances, which is absurd. Therefore, the temperatures continue equal.

It follows, that the quantity of heat in unity of weight of a substance at a given temperature, may be expressed by the product of a quantity depending on the nature of the substance, and independent of the temperature, multiplied by a function of the temperature, which is the same for all substances.

Let τ denote the temperature of a body according to the scale adopted; κ , the position, on the same scale, of the temperature corresponding to absolute privation of heat; k , a quantity depending on the nature of the substance, and independent of temperature. Then the quantity of heat in unity of weight may be expressed as follows:—

$$Q = k (\psi \cdot \tau - \psi \cdot \kappa) \quad (81.)$$

(54.) If we introduce this notation into the formula (79) which expresses the proportion of the total heat expended, which is converted into useful power by an expansive machine working to the best advantage, the quantity k , peculiar to the substance employed, disappears, and we obtain **CARNOT'S THEOREM**, as modified by Messrs **CLAUSIUS** and **THOMSON**, viz.,—that this ratio is a function solely of the temperatures at which heat is received and emitted respectively, and is independent of the nature of the substance; or symbolically,

$$\frac{\text{Effect}}{\text{Heat Expended}} = \frac{Q_1 - Q_2}{Q_1} = \frac{\psi \cdot \tau_1 - \psi \cdot \tau_2}{\psi \cdot \tau_1 - \psi \cdot \kappa} \quad (82.)$$

(55.) Let us now apply the same notation to the formula (67) for the latent heat of a small expansion dV at constant heat, viz:—

$$Q \frac{dP}{dQ} \cdot dV$$

we have evidently

$$\frac{dP}{dQ} = \frac{1}{\frac{dQ}{d\tau}} \cdot \frac{dP}{d\tau} = \frac{1}{k \psi \cdot \tau} \cdot \frac{dP}{d\tau}$$

and consequently, the heat which disappears by the expansion dV is

$$Q \frac{dP}{dQ} \cdot dV = \frac{\psi \cdot \tau - \psi \cdot \kappa}{\psi \cdot \tau} \cdot \frac{dP}{d\tau} \cdot dV \quad (83.)$$

from which formula the specific quantity k has disappeared.

Now, in the notation of Professor **THOMSON** we have

$$\frac{\psi \cdot \tau - \psi \cdot \kappa}{\psi \cdot \tau} = \frac{J}{\mu}$$

where J is JOULE's equivalent, and μ a function of the temperature, the same for all substances, to be determined empirically; and consequently,

$$\begin{aligned} \text{hyp. log. } (\psi \cdot \tau - \psi \cdot \kappa) &= \frac{1}{J} \int^{\tau} \mu d\tau \\ \text{or, } \psi \cdot \tau - \psi \cdot \kappa &= \epsilon \frac{1}{J} \int^{\tau} \mu d\tau \\ \text{and } Q &= \kappa (\psi \cdot \tau - \psi \cdot \kappa) = \kappa \cdot \epsilon \frac{1}{J} \int^{\tau} \mu d\tau \end{aligned} \quad \left. \vphantom{\begin{aligned} \text{hyp. log. } (\psi \cdot \tau - \psi \cdot \kappa) &= \frac{1}{J} \int^{\tau} \mu d\tau \\ \psi \cdot \tau - \psi \cdot \kappa &= \epsilon \frac{1}{J} \int^{\tau} \mu d\tau \\ Q &= \kappa (\psi \cdot \tau - \psi \cdot \kappa) = \kappa \cdot \epsilon \frac{1}{J} \int^{\tau} \mu d\tau \end{aligned}} \right\} \quad (84.)$$

These expressions will be recognised by those who have studied Professor THOMSON's papers on the Dynamical Theory of Heat. By introducing the value given above of the quantity of heat in unity of weight, into the formulæ of the preceding articles of this section, they are at once transformed to those of Professor THOMSON, and in particular, the formulæ 79 and 82 become the following:—

$$\frac{\text{* Effect of Machine}}{\text{Heat Expended}} = \frac{\epsilon \frac{1}{J} \int^{\tau_1} \mu d\tau - \epsilon \frac{1}{J} \int^{\tau_2} \mu d\tau}{\epsilon \frac{1}{J} \int^{\tau_1} \mu d\tau} = 1 - \epsilon \frac{1}{J} \int^{\tau_2} \mu d\tau \quad (85.)$$

SUB-SECTION 3.—On the Hypothesis of Molecular Vortices.

(56.) The use of a Mechanical Hypothesis in the Theory of Heat, as in other branches of physics, is to render it a branch of Mechanics, the only complete physical science; and to deduce its principles from the laws of Force and Motion, which are better understood than those of any other phenomena.

The results of the investigations in the preceding part of this section are consistent alike with all conceivable hypotheses which ascribe the phenomena of heat to invisible motions amongst the particles of bodies.

Those investigations, however, leave undetermined the relation between temperature and quantity of heat, except in so far as they shew that it must follow the same law of variation in all substances.

By adopting a definite hypothesis, we are conducted to a definite relation between temperature and quantity of heat; which, being introduced into the formulæ, leads to specific results respecting the phenomena of the mutual transformation of heat and visible mechanical power; and those results, being compared with experiment, furnish a test of the soundness of the hypothesis.

Thus the hypothesis of Molecular Vortices, which forms the basis of the investigations in the first five sections of this paper, and in a paper on the Centrifugal Theory of Elasticity, leads to the conclusion, that, if temperature be mea-

* It is to be observed, that in Professor THOMSON's notation, heat is supposed to be measured by an arbitrary unit, whose ratio to a unit of mechanical power is denoted by J ; while in this paper, the same unit is employed in expressing quantities of heat and of mechanical power.

sured by the expansion of a perfect gas, the total quantity of heat in a body is simply proportional to the elevation of its temperature above the temperature of absolute privation of heat; or, in the notation of the preceding article,

$$\psi \cdot \tau = \tau, \quad \psi' \cdot \tau = 1, \quad \text{and} \\ Q = k (\tau - \kappa) \quad (86.)$$

k being the real specific heat of the body.

If this value be substituted for the quantity of heat Q , in all the formulæ, from 67 to 80 inclusive, which are founded simply on the definition of expansive heat, it reproduces all the formulæ which, in this and the other paper referred to, have been deduced directly from the hypothesis. In the sequel I shall apply one of these formulæ to the calculation, from the experiments of Professor THOMSON and Mr JOULE on the heating of currents of air by friction, of approximate values of the absolute temperature corresponding to total privation of heat, that the mutual consistency of those values may serve as a test of the soundness of the hypothesis, and the accuracy of the formulæ deduced from it.

(57.) Before proceeding further, it may be desirable to point out how far this hypothesis agrees with, and how far it differs from, that proposed by Mr HERAPATH and Mr WATERSTON, which supposes bodies to consist of extremely small and perfectly elastic particles, which fly about in all directions with a velocity whose half-square is the mechanical equivalent of the heat possessed by unity of weight, and are prevented from dispersing by their collisions with each other and with the particles of surrounding bodies. Let v be the velocity of motion, then

$$\frac{v^2}{2g} = Q$$

represents the heat possessed by unity of weight, expressed in terms of the force of gravity.

The expansive pressure due to such motions is found by conceiving a hard, perfectly elastic plane of the area unity to be opposed to the collision of the particles, and calculating the pressure which would be required to maintain its position against them. If all the particles were to strike and rebound from such a plane at right angles, the pressure would be represented thus:

$$\frac{v^2}{g} \cdot \frac{1}{V}$$

where V is the volume which contains so many particles as amount to unity of weight. But the particles are supposed to fly in equal numbers in all directions. Then if θ denote the angle of incidence on the plane

$$\frac{\sin \theta d\theta}{\int_0^{\frac{\pi}{2}} \sin \theta d\theta} = \sin \theta d\theta$$

represents the proportion of the whole particles which fly in those directions which make the angle θ with the normal to the plane. Of this proportion, again,

the fraction $\cos \theta$ only strikes the plane; while the force of the blow also is less than that of a normal blow in the ratio $\cos \theta : 1$. Hence the mean force of collision is

$$\int_0^{\frac{\pi}{2}} \cos^2 \theta \sin \theta d\theta = \frac{1}{3}$$

of the force of a perpendicular collision; so that the expansive pressure is represented by

$$\frac{1}{3} \cdot \frac{v^2}{g} \cdot \frac{1}{V} = \frac{2}{3} \cdot \frac{Q}{V} = P$$

Hence, according to this hypothesis, we should have for a perfect gas

$$PV = \frac{2}{3} Q$$

or the product of the pressure and volume of a mass of a perfect gas equal to two-thirds of the mechanical equivalent of its total heat.

It is known, however, that the product of the pressure and volume of a mass of sensibly perfect gas is only about four-tenths of the equivalent of its total heat. The hypothesis, therefore, requires modification.

By supposing the particles to attract each other, or to be of appreciable bulk compared with the distances between them, the ratio in question is diminished; but either of these suppositions is inconsistent with the perfectly gaseous condition.

It appears to me, that, besides this difficulty connected with the gaseous condition, there exists also great difficulty in conceiving how the hypothesis can be applied to the solid condition, in which the particles preserve definite arrangements. The limited amount of time and attention, however, which I have hitherto bestowed on this hypothesis, is not sufficient to entitle me to pronounce whether these difficulties admit of a solution.

(58.) The idea of ascribing expansive elasticity to the centrifugal force of vortices or eddies in elastic atmospheres surrounding nuclei of atoms, originated with Sir HUMPHRY DAVY. The peculiarity of the view of the hypothesis taken in this paper consists in the function ascribed to the nuclei or central physical points of the atoms, which, besides retaining the atmospheres round them by their attraction, are supposed, by their actions on each other, to constitute the medium which transmits radiant heat and light; so that heat is radiant or thermometric, according as it affects the nuclei or their atmospheres.

In this form the hypothesis of Molecular Vortices is not a mere special supposition, to elucidate the theory of expansive heat, but becomes connected with the theory of the elasticity of matter in all conditions, from solid to gaseous, and with that of the transmission of radiations.

I have already investigated mathematically the consequences of this hypothesis by two different processes, which are necessarily somewhat complicated.

When the question, however, is confined to the relations between temperatures and quantities of heat, a more simple process may be followed, analogous to that which has been applied in the preceding article to the hypothesis of Molecular Collisions.

If a mass of elastic fluid, so much rarefied that the effect of molecular attraction is insensible, be entirely filled with vortices, eddies, or circulating currents of any size and figure, so that every particle moves with the common velocity w , then, if the planes of revolution of these eddies be uniformly distributed in all possible positions, it follows, from reasoning precisely similar to that employed in the preceding article, that the pressure exerted by the fluid against a plane, in consequence of the centrifugal force of the eddies, has the following value in terms of gravity:—

$$\frac{1}{3} \cdot \frac{w^2}{g} \cdot \frac{1}{V} \quad \dots \dots \dots (87.)$$

or two-thirds of the hydrostatic pressure due to the velocity of the eddies w ; V being, as before, the volume occupied by unity of weight.

It is, however, reasonable to suppose, that the motion of the particles of atomic atmospheres does not consist merely in circulating currents; but that those currents are accompanied with a certain proportionate amount of vibration,—a kind of motion which does not produce centrifugal force. To these we have to add the oscillations of the atomic nuclei, in order to obtain the mechanical equivalent of the whole molecular motions; which is thus found to be expressed for unity of weight by

$$k \frac{w^2}{2g} = Q \quad \dots \dots \dots (88.)$$

k being a specific coefficient. Hence it follows (denoting $\frac{2k}{3}$ by N), that the expansive pressure due to molecular motions in a perfect gas, is equal to the mechanical equivalent of those motions in unity of volume multiplied by a specific constant

$$N \cdot \frac{Q}{V} \quad \dots \dots \dots (89.)$$

The coefficient N has to be determined by experiment; its value for atmospheric air is known to be between 0.4 and 0.41.

In order to account for the transmission of pressure throughout the molecular atmospheres, it is necessary to suppose them possessed of a certain amount of inherent elasticity, however small, varying proportionally to density, and independent of heat. Let this be represented by

$$\frac{h}{V}$$

then

$$P = (NQ + h) \frac{1}{V} \quad \dots \dots \dots (90.)$$

is the total pressure of a perfect gas.

Equilibrium of heat and pressure between portions of two different perfect gases in contact requires that the pressures independent of heat, and the pressures caused by heat, shall separately be in equilibrio. Let the suffixes a and b be used to distinguish quantities relative to two different substances in the perfectly gaseous condition. Then the first condition of equilibrium is expressed as follows:—

$$\left(\frac{h}{V}\right) (a) = \left(\frac{h}{V}\right) (b) \quad . \quad . \quad . \quad . \quad . \quad . \quad (91.)$$

that is to say, *the densities of two perfect gases in equilibrio are inversely proportional to the coefficients of elasticity of their atomic atmospheres.*

The second condition is expressed as follows:—

$$\left(\frac{NQ}{V}\right) (a) = \left(\frac{NQ}{V}\right) (b)$$

which, being taken in connection with the first condition, gives —

$$\left(\frac{NQ}{h}\right) (a) = \left(\frac{NQ}{h}\right) (b) \quad . \quad . \quad . \quad . \quad . \quad . \quad (92.)$$

Now by Equation 90, we have

$$\frac{NQ}{h} = \frac{PV}{h} - 1$$

Hence the condition of equilibrium of heat between two perfect gases is

$$\left(\frac{PV}{h}\right) (a) = \left(\frac{PV}{h}\right) (b) \quad . \quad . \quad . \quad . \quad . \quad . \quad (93.)$$

consequently, *temperature may be measured by the product of the pressure and volume of a perfect gas, divided by a coefficient, which is proportional to the volume of the gas at a standard pressure and temperature.*

Temperatures thus measured are reckoned from the point known as the *zero of gaseous tension*, or *absolute zero of a perfect gas thermometer*, $274^{\circ}6$ centigrade below the temperature of melting ice.

Let V_0 denote the volume of unity of weight of a perfect gas, at a standard pressure P_0 , and absolute temperature τ_0 ; then any other absolute temperature has the following value:—

$$\tau = \tau_0 \frac{PV}{P_0 V_0} = \frac{\tau_0}{P_0 V_0} (NQ + h) \quad . \quad . \quad . \quad . \quad . \quad . \quad (94.)$$

while the absolute temperature of total privation of heat is

$$\kappa = \tau_0 \frac{h}{P_0 V_0} \quad . \quad . \quad . \quad . \quad . \quad . \quad (94 A.)$$

Hence it appears that quantity of heat in unity of weight bears the following relation to temperature,—

$$Q = \frac{1}{N} (PV - h) = \frac{P_0 V_0}{N \tau_0} (\tau - \kappa) \quad . \quad . \quad . \quad . \quad . \quad . \quad (95.)$$

in which, if we substitute the symbol of real specific heat,

$$k = \frac{P_0 V_0}{N \tau_0} \quad (96.)$$

we obtain the formula already given (86) for the relation between heat and temperature.*

(59.) The introduction of the value given above of the quantity of heat in terms of temperature, into the formula 67, gives for the latent heat of a small expansion dV at constant temperature

$$(\tau - \kappa) \frac{dP}{d\tau} \cdot dV \quad (97.)$$

The formulæ 79 and 82, for the proportion of heat rendered available by an expansive engine working to the greatest advantage, becomes

$$\frac{\tau_1 - \tau_2}{\tau_1 - \kappa} \quad (98.)$$

or the ratio of the difference between the temperatures of receiving and emitting heat, to the elevation of the former temperature above that of total privation of heat. This is the law already arrived at by a different process in Section V. of this paper.

When the same substitution is made in Equation 80, which represents the total energy, whether as heat or as compressive power, which must be applied to unity of weight of a substance to produce given changes of heat and volume, the following result is obtained:—

$$\begin{aligned} d \cdot \Psi = dQ + d \cdot S &= \left\{ k + f'(\tau) + (\tau - \kappa) \int \frac{d^2 P}{d\tau^2} d\tau \right\} d\tau \\ &+ \left\{ (\tau - \kappa) \frac{dP}{d\tau} - P \right\} dV \\ &= d \cdot \left\{ k \tau + f(\tau) + \left((\tau - \kappa) \frac{d}{d\tau} - 1 \right) \int P dV \right\} \quad (99.) \end{aligned}$$

As it cannot be simplified, it is unnecessary here to recapitulate the investigation, which leads to the conclusion that the functions $f(\tau)$ and $f'(\tau)$ have the following values:—

$$f(\tau) = k N \left(\kappa \text{ hyp. log. } \tau + \frac{\kappa^2}{\tau} \right); f'(\tau) = k N \left(\frac{\kappa}{\tau} - \frac{\kappa^2}{\tau^2} \right) \quad (99 A.)$$

We have thus reproduced Equation 26 of the paper formerly referred to, on the Centrifugal Theory of Elasticity.

The coefficient of the variation of temperature in the first form of Equation 99 is the specific heat of the substance at constant volume. Denoting this by K_v , the formula becomes

$$d \cdot \Psi = K_v \cdot d\tau + \left\{ (\tau - \kappa) \frac{dP}{d\tau} - P \right\} dV \quad (100.)$$

* See Appendix, Note A.

SUB-SECTION 4. *Thermic Phenomena of Currents of Elastic Fluids.*

(60.) When a gas previously compressed is allowed to escape through small apertures, as in the experiments of Mr JOULE and Professor THOMSON, and has its velocity destroyed entirely by the mutual friction of its particles, without impediment from any other substance, and without conduction of heat to or from any other substance; then its condition is expressed by making

$$d\psi = 0$$

that is to say,

$$-d\tau = \frac{1}{K_v} \cdot \left\{ \tau \cdot \left(\frac{dP}{d\tau} - \frac{P}{\tau} \right) - \kappa \frac{dP}{d\tau} \right\} dV \quad (101.)$$

If we assume (as is really the case in the experiments) that the specific heat of the gas at constant volume does not sensibly vary within the limits of the experiments as to temperature and volume, so that K_v is sensibly constant, and also that the variation of temperature is very small as compared with the absolute temperatures, then we have the following approximate integral:—

$$-\Delta\tau = \frac{1}{K_v} \cdot \left\{ \tau \int_{V_1}^{V_2} \left(\frac{dP}{d\tau} - \frac{P}{\tau} \right) dV - \kappa \int_{V_1}^{V_2} \frac{dP}{d\tau} \cdot dV \right\} \quad (102.)$$

which represents the cooling effect of an expansion from the volume V_1 to the volume V_2 .

If it were possible to obtain any substance in the state of perfect gas to be used in experiments of this kind, the first integral in the above expression would disappear, because, for a perfect gas,

$$\frac{dP}{d\tau} = \frac{P}{\tau}$$

and as the other term is negative, the result would be a slight heating effect. As no gas, however, is perfect, and as $\frac{dP}{d\tau}$ always exceeds $\frac{P}{\tau}$, the mode of reducing the experimental data is to calculate the value of the first term, which represents the effect of cohesion, from the known properties of the gas, to subtract from it the actual cooling, and from the remainder to compute values of κ , the temperature of absolute privation of heat, according to the following formula:—

$$\kappa = \frac{\frac{1}{K_v} \tau \int_{V_1}^{V_2} \left(\frac{dP}{d\tau} - \frac{P}{\tau} \right) dV - (-\Delta\tau)}{\frac{1}{K_v} \int_{V_1}^{V_2} \frac{dP}{d\tau} dV} \quad (103.)$$

When the gas is nearly perfect, as in the case of atmospheric air, it is unnecessary to take into consideration its deviation from the perfect condition in computing the integral in the denominator; whose approximate value is found to be

$$\frac{P_0 V_0}{K_v \tau_0} \cdot \text{hyp. log } \frac{V}{V_1} = N \cdot \text{hyp. log } \frac{P}{P_1} \text{ nearly } (\tau \text{ being nearly constant}),$$

and $K_v \text{ nearly} = k$.

The value of the integral in the numerator is found as follows:—

The Centrifugal Theory of Elasticity indicates that the pressure of an imperfect gas may be represented by the following formula:—

$$P = P_0 \frac{V_0}{V} \left\{ \frac{\tau}{\tau_0} + A_0 - \frac{A_1}{\tau} - \frac{A_2}{\tau^2} - \&c. \right\} \quad (104.)$$

where V_0 is the volume in the perfectly gaseous state, at a standard pressure P_0 , and absolute temperature τ_0 , and $A_0, A_1, \&c.$, are a series of functions of the density, to be determined empirically. From this formula it is easily seen that

$$\frac{dP}{d\tau} - \frac{P}{\tau} = P_0 \frac{V_0}{V} \left\{ -\frac{A_1}{\tau^2} + \frac{2A_2}{\tau^3} + \&c. \right\} \quad (105.)$$

so that the first term in the numerator of the expression (103) has the following value:—

$$\frac{\tau}{K_v} \int_{V_1}^{V_0} \left(\frac{dP}{d\tau} - \frac{P}{\tau} \right) dV = \frac{P_0 V_0}{K_v} \left\{ -\int_{V_1}^{V_0} \frac{A_1}{V} dV + \frac{2}{\tau} \int_{V_1}^{V_0} \frac{A_2}{V} dV + \&c. \right\} \quad (106.)$$

in which $\frac{P_0 V_0}{K_v} = N \tau_0$ nearly.

In order to represent correctly the result of M. REGNAULT'S experiments on the elasticity and expansion of gases, it was found sufficient to use, in the formula for the pressure (104), the first three terms; and the functions of the density which occur in these terms, as determined empirically from the experiments, were found to have the following values, in which the unit of volume is the theoretical volume of unity of weight of air under the pressure of one atmosphere, at the temperature of melting ice,* and the values of the constants are given for the centigrade scale.

$$\frac{A_2}{V} = b \left(\frac{1}{V} \right)^{\frac{1}{2}}; \quad \frac{A_1}{V} = a \left(\frac{1}{V} \right)^{\frac{1}{2}} \quad (107.)$$

Com. log $b = 3.8181545$; Com. log $a = 0.3176168$.

Hence it appears that the integrals in the formula (106) have the following values:—

$$\int_{V_1}^{V_0} \frac{A_2}{V} dV = 2b \cdot \Delta \cdot \left(\frac{1}{V} \right)^{\frac{1}{2}}; \quad \frac{2}{\tau} \int_{V_1}^{V_0} \frac{A_1}{V} dV = \frac{10}{3} \cdot \frac{a}{\tau_0} \cdot \frac{\tau_0}{\tau} \cdot \Delta \cdot \left(\frac{1}{V} \right)^{\frac{1}{2}} \quad (107A.)$$

in which the common logarithms of the constants are

* This unit of volume is greater than the actual volume of air, under the circumstances described, in the ratio of 1.00085 to 1.

$$\text{Com. log } 2b = 2.1101845; \log \frac{10}{3} \cdot \frac{a}{\tau_0} = 2.4017950;$$

and these values suit any scale of temperatures.

In calculating, for use in these formulæ, the densities $\frac{1}{V}$ from the observed pressures, it is sufficiently near the truth, in the case of air, to use the approximate equation

$$\frac{1}{V} = \frac{\tau_0}{\tau} \cdot P \text{ (in atmospheres).}$$

The common logarithm of τ_0 , the absolute temperature of melting ice, for the centigrade scale, is 2.4387005.

The constant N for atmospheric air is 0.4 nearly; therefore

$$\text{Com. log } (N \times \text{hyp. log } 10) = 1.9642757.$$

The following, therefore, is the approximate value of the formula (103), to be used (with the numerical constants already given) in reducing the experiments of Mr JOULE and Professor THOMSON on atmospheric air, so as to obtain approximate values of the absolute temperature of total privation of heat:—

$$\kappa = \left\{ N \tau_0 \left(\frac{10a}{3\tau_0} \cdot \left(\frac{\tau_0}{\tau} \right)^{\frac{11}{10}} \Delta \cdot (P^{\frac{1}{10}}) - 2b \left(\frac{\tau_0}{\tau} \right)^{\frac{1}{10}} \Delta \cdot (P^{\frac{1}{10}}) \right) - (-\Delta \tau) \right\} \\ \div N \text{ hyp. log } 10 \times \Delta \cdot \text{com. log } \frac{1}{P} \quad \dots \dots \dots (108.)$$

In using this formula, the mean absolute temperature should be taken as the value of τ .

The following table shews the values of the quantity κ , computed from ten mean experimental data, taken respectively from the first ten series of experiments described in the recent paper of Messrs JOULE and THOMSON, in the supplementary number of the *Philosophical Magazine* for December 1852. The temperatures in the table, for the sake of convenience, are reduced to the centigrade scale, because that scale has been used throughout the previous sections of this paper.

The final pressure in each case was that of the atmosphere.

SERIES AND NUMBER OF EXPERIMENTS.	Quantity of Air used in cubic inches per second.	Initial Temperature, Centigrade.	Mean Absolute Temperature τ .	Initial Pressure in Atmospheres.	Actual Cooling, Centigrade.	Calculated Cooling by Cohesion, Centigrade.	Difference.	Approximate Values of τ , the Absolute Temperature of Total Privation of Heat, Centigrade.
Series I. Mean of 3 Experiments, }	Not noted.	16.11	290.4	3.61	0.76	1.70	0.94	1.83
Series II. Mean of 5 Experiments, }	Not noted.	71.11	345.6	2.64	0.28	0.70	0.42	1.08
Series III. Mean of 7 Experiments, }	6.4	77.11	351.5	4.28	0.37	1.25	0.88	1.51
Series IV. Mean of 3 Experiments, }	5.6	17.39	281.7	3.04	0.56	1.49	0.93	2.09
Series V. Mean of 2 Experiments, }	8.4	3.89	278.0	4.11	1.00	2.18	1.18	2.087
Series VI. Mean of 4 Experiments, }	11.2	5.21	279.4	4.40	0.92	2.31	1.39	2.345
Series VII. Mean of 5 Experiments, }	1.4	17.87	292.3	1.894	0.23	0.66	0.43	1.683
Series VIII. Mean of 5 Experiments, }	2.8	19.04	292.5	2.41	0.31	0.93	0.62	1.762
Series IX. Mean of 4 Experiments, }	5.6	18.31	292.6	2.92	0.43	1.385	0.955	2.228
Series X. Mean of 3 Experiments, }	11.2	18.37	292.6	4.11	0.72	1.93	1.21	2.14

Professor THOMSON and Mr JOULE have expressed the opinion, which is undoubtedly correct, that those experiments in which the largest quantities of air were used were the least liable to error from disturbing causes, such as the conduction of heat.

Now it may be observed in the preceding table, that the calculated values of κ are generally greatest, and the discrepancies amongst them least, for the experiments in which most air was used. To illustrate this, the results of the last eight series are arranged below in the order of the quantities of air employed.

Cubic inches per second,	1.4	2.8	5.6	5.6	6.4	8.4	11.2	11.2
Values of κ , ...	1.683	1.762	2.09	2.228	1.51	2.087	2.345	2.14

It is further to be remarked, that the discrepancy between the highest and the lowest of the values of κ is

$$2^{\circ}345 - 1^{\circ}08 = 1^{\circ}265 \text{ centigrade:}$$

a quantity which corresponds to a difference of less than *one three-hundredth part* in computing the proportion of heat converted into mechanical power by any ordinary expansive engine, according to the formula (98), which has been deduced from the hypothesis of Molecular Vortices.

The experiments, therefore, may be considered as tending to prove, that the formulæ deduced from this hypothesis are sufficiently correct for practical purposes; and also as affording a strong probability that the principles to which it leads are theoretically exact, and that the temperature of absolute privation of heat is a real fixed point on the scale, somewhat more than two centigrade degrees above the absolute zero of a perfect gas-thermometer (which is, of course, an imaginary point); that is to say, about $272\frac{1}{2}$ centigrade degrees, or $490\frac{1}{2}$ degrees of FAHRENHEIT, below the temperature of melting ice.

If these conclusions be correct, it follows, that when the temperatures T_1 and T_2 , between which an expansive engine works, are measured from the ordinary zero points of the centigrade and of FAHRENHEIT's scales respectively, the following are the utmost proportions of the total heat expended which it can be made to convert into mechanical power:—

$$\left. \begin{array}{l} \text{For the centigrade scale, } \frac{T_1 - T_2}{T_1 + 272\frac{1}{2}} \\ \text{For FAHRENHEIT's scale, } \frac{T_1 - T_2}{T_1 + 458\frac{1}{2}} \end{array} \right\} \quad (109.)$$

In the fifth section of this paper, where a comparison is made between the actual duty of the Cornish engine at Old Ford, as determined by Mr WICKSTEED, and the greatest possible duty which could be obtained from a given quantity of heat by a theoretically perfect engine working between the same temperatures, the constant κ is treated as being so small that it may be neglected in practice.

If the value of κ is really $2^{\circ}\cdot 1$ centigrade, as computed above, the calculated maximum theoretical duty in Section V. is too small by about one one-hundred-and-ninetieth part of its amount,—a quantity of no practical importance in such calculations.

(61.) It may be anticipated, that when Mr JOULE and Professor THOMSON shall have performed experiments on the thermic phenomena exhibited by air in more copious currents, and by gases of more definite composition, and more simple laws of elasticity, much more precise results will be obtained.

When a gas deviating considerably from the perfectly gaseous condition, or a vapour near the point of saturation, is employed, it will no longer be sufficiently accurate to treat the specific heat at constant volume as a constant quantity, nor the cooling effect as very small. It will therefore be necessary to employ, for the reduction of the experiments, the integral form of equation (99); that is to say,

$$\begin{aligned} 0 = \Delta \Psi = \Delta \left\{ \kappa \tau + \kappa N \kappa \left(\text{hyp. log } \tau + \frac{\kappa}{\tau} \right) + \left((\tau - \kappa) \frac{d}{d\tau} - 1 \right) \int P dV \right\} \\ = \kappa (\tau_2 - \tau_1) + \Delta \int \left(\tau \frac{dP}{d\tau} - P \right) dV \\ - \kappa \left\{ \Delta \int \frac{dP}{d\tau} dV - \kappa N \left(\Delta \cdot \frac{\kappa}{\tau} + \Delta \text{ hyp. log } \tau \right) \right\} \quad (110.) \end{aligned}$$

(62.) Preliminary to the application of this equation, it is necessary to determine the mechanical value of the real specific heat κ . Supposing the law which connects the pressure, density, and temperature of the gas to be known, it is sufficient for this purpose to have an accurate experimental determination, either of the apparent specific heat at constant pressure for a given temperature, or the velocity of sound in the gas under given circumstances.

First, let us suppose that the apparent specific heat at constant pressure is known.

The value of this coefficient (Centrifugal Theory of Elasticity, art. 12) is

$$K_r = \kappa + (\tau - \kappa) \left\{ \frac{P_0 V_0}{\tau_0} \frac{\kappa}{\tau^2} + \int \frac{d^2 P}{d\tau^2} dV + \frac{\left(\frac{dP}{d\tau} \right)^2}{-\frac{dP}{dV}} \right\} \quad (111.)$$

In order that the lower limit of the integral may correspond with the condition of perfect gas, it is convenient to transform it into one in terms of the density. Let D be the weight of unity of volume, then

$$\int \frac{d^2 P}{d\tau^2} dV = - \int_0^D \frac{1}{D^2} \cdot \frac{d^2 P}{d\tau^2} dD \quad (111 A.)$$

If, then, we have the pressure of the gas under consideration expressed by the following approximate formula:—

$$P = \frac{P_0 V_0}{V} \left\{ \frac{\tau}{\tau_0} + A_0 - \frac{A_1}{\tau} \right\}$$

The following will be the values of the functions of the pressure which enter into the above equation:—

$$\left. \begin{aligned} \frac{dP}{d\tau} &= \frac{P_0 V_0}{V} \left\{ \frac{1}{\tau_0} + \frac{A_1}{\tau^2} \right\}; \quad \frac{d^2 P}{d\tau^2} = -2 \frac{P_0 V_0}{V} \cdot \frac{A_1}{\tau^3} = -2 P_0 V_0 \frac{A_1 D}{\tau^3} \\ \int \frac{d^2 P}{d\tau^2} dV &= - \int_0^D \frac{1}{D^2} \cdot \frac{d^2 P}{d\tau^2} dD = + 2 \frac{P_0 V_0}{\tau^3} \cdot \int_0^D \frac{A_1}{D} dD; \\ - \frac{dP}{dV} &= D^2 \frac{dP}{dD} = P_0 V_0 D^2 \left\{ \frac{\tau}{\tau_0} + \frac{d \cdot A_0 D}{dD} - \frac{1}{\tau} \cdot \frac{d \cdot A_1 D}{dD} \right\} \\ \therefore \frac{\left(\frac{dP}{d\tau} \right)^2}{-\frac{dP}{dV}} &= P_0 V_0 \cdot \frac{\left(\frac{1}{\tau_0} + \frac{A_1}{\tau^2} \right)^2}{\frac{\tau}{\tau_0} + \frac{d \cdot A_0 D}{dD} - \frac{1}{\tau} \cdot \frac{d \cdot A_1 D}{dD}} \end{aligned} \right\} \quad (111 B.)$$

To illustrate the application of these formulæ, let us calculate the difference between the real specific heat, and the apparent specific heat, at constant pressure, of carbonic acid gas, at the temperature of melting ice, and at the density which, if the gas were perfect, would correspond to a pressure of one atmosphere at the temperature of melting ice. Let this density be denoted by D_0 , and its reciprocal by V_0 . As the constants have been deduced from M. REGNAULT's experiments, the calculations will be made in French measures and for the latitude of Paris.

The actual density of carbonic acid at 0° centigrade, and under one atmosphere of pressure, exceeds the theoretical density, in the perfectly gaseous state, in the ratio of 1.0065 to 1 nearly. Hence the height of a homogeneous atmosphere of actual carbonic acid at 0° centigrade being 5225.5 metres, the corresponding height in the state of perfect gas is $P_0 V_0 = 5259.5$ „

and $\frac{P_0 V_0}{\tau} = 19.53$ metres per centigrade degree = 62.84 feet.

The functions which express the influence of density on the deviation of carbonic acid gas from the perfectly gaseous state, have the following values:—

$$\left. \begin{aligned} A_0 &= b \cdot \frac{D}{D_0}; \quad A_1 = a \cdot \frac{D}{D_0}; \quad \text{when} \\ \text{Com. log } b &= \bar{3}.1083932; \quad \text{Com. log } a = 0.3344538 \\ b &= 0.00128349 \quad a = 2.16; \\ \int_0^D \frac{A_1}{D} dD &= \int_0^D A_1 \frac{D_0}{D} \cdot \frac{d \cdot D}{D_0} = a \cdot \frac{D}{D_0}; \quad \frac{d}{dD} \cdot A_0 D \\ &= 2b \cdot \frac{D}{D_0}; \quad \frac{d}{dD} \cdot A_1 D = 2a \frac{D}{D_0} \end{aligned} \right\} \quad (111 C.)$$

For the purposes of a first approximation, we may assume that the value of κ

already found is sufficiently near the truth, viz., $2^{\circ}\cdot 1$ centigrade; so that, in the present instance $\tau - \kappa = 272^{\circ}\cdot 5$ centigrade.

Then we find the following results when $\tau = \tau_0$, and $D = D_0$:

	Metres.	Feet.
$(\tau - \kappa) \frac{P_0 V_0}{\tau_0} \cdot \frac{\kappa}{\tau^2} =$ per centigrade degree, ...	0.145	0.48
$(\tau - \kappa) \int \frac{d^2 P}{d\tau^2} dV =$ " " "	0.150	0.49
Sum = $K_v - k$ = excess of apparent specific heat at constant volume above real specific heat, ...	0.295	0.97
$(\tau - \kappa) \frac{\left(\frac{dP}{d\tau}\right)^2}{-\frac{dP}{dV}} =$ difference between apparent specific heats at constant volume and at constant pressure, ...	19.565	64.19
$K_p - k$ = excess of apparent specific heat at constant pressure above real specific heat, ...	19.860	65.16

$\frac{5}{9}$ of the above quantities are of course the corresponding quantities for FAHRENHEIT's scale.

Secondly, If the velocity of sound in the gas is given, let this = u . Then we know that

$$u^2 = g \cdot \frac{dP}{dD} \cdot \frac{K_p}{K_v} \quad (112.)$$

in which

$$\frac{dP}{dD} = P_0 V_0 \left\{ \frac{\tau}{\tau_0} + \frac{d \cdot A_0 D}{dD} - \frac{1}{\tau} \frac{d \cdot A_1 D}{dD} \right\} \quad (112 A.)$$

So that from the velocity of sound we can calculate the ratio of the specific heats at constant pressure and at constant volume. Let this ratio be denoted by γ , and let

$$K_v = k + c; \quad K_p = k + c'; \quad \text{then}$$

$$\gamma = \frac{k + c'}{k + c}; \quad \text{and } k = \frac{c' - \gamma c}{\gamma - 1} \quad (112 B.)$$

in which c' and c are to be calculated as above.*

(63.) In using the formula (110) for a gas whose pressure is represented by the formula,

$$P = \frac{P_0 V_0}{V} \left\{ \frac{\tau}{\tau_0} + A_0 - \frac{A_1}{\tau} \right\}$$

the integrals may be transformed so as to be taken, with respect to the density, as in the preceding article. Thus we obtain

* See Appendix, Note B.

$$\Delta \int \left(\tau \frac{dP}{d\tau} - P \right) dV = -\Delta \int \frac{1}{D^2} \left(\tau \frac{dP}{d\tau} - P \right) dD = -P_0 V_0 \Delta \left\{ \frac{2}{\tau} \int_0^D \frac{A_1}{D} dD - \int \frac{A_0}{D} dD \right\}$$

$$\int \frac{dP}{d\tau} dV = -\int \frac{1}{D^2} \cdot \frac{dP}{d\tau} dD = -P_0 V_0 \Delta \left(\frac{1}{\tau_0} \text{hyp. log } D + \frac{1}{\tau^2} \int \frac{A_1}{D} dD \right) \quad (113.)$$

For carbonic acid, the first of these formulæ becomes simply

$$\left. \begin{aligned} &+ P_0 V_0 \left\{ \frac{2a}{D_0} \left(\frac{D_1}{\tau_1} - \frac{D_2}{\tau_2} \right) - \frac{b}{D_0} (D_1 - D_2) \right\} \\ \text{and the second,} \\ &+ P_0 V_0 \left\{ \frac{1}{\tau_0} \text{hyp. log } \frac{D_1}{D_2} - \frac{a}{D_0} \left(\frac{D_1}{\tau_1^2} - \frac{D_2}{\tau_2^2} \right) \right\} \end{aligned} \right\} \quad (113 A.)$$

GLASGOW, 27th December 1852.

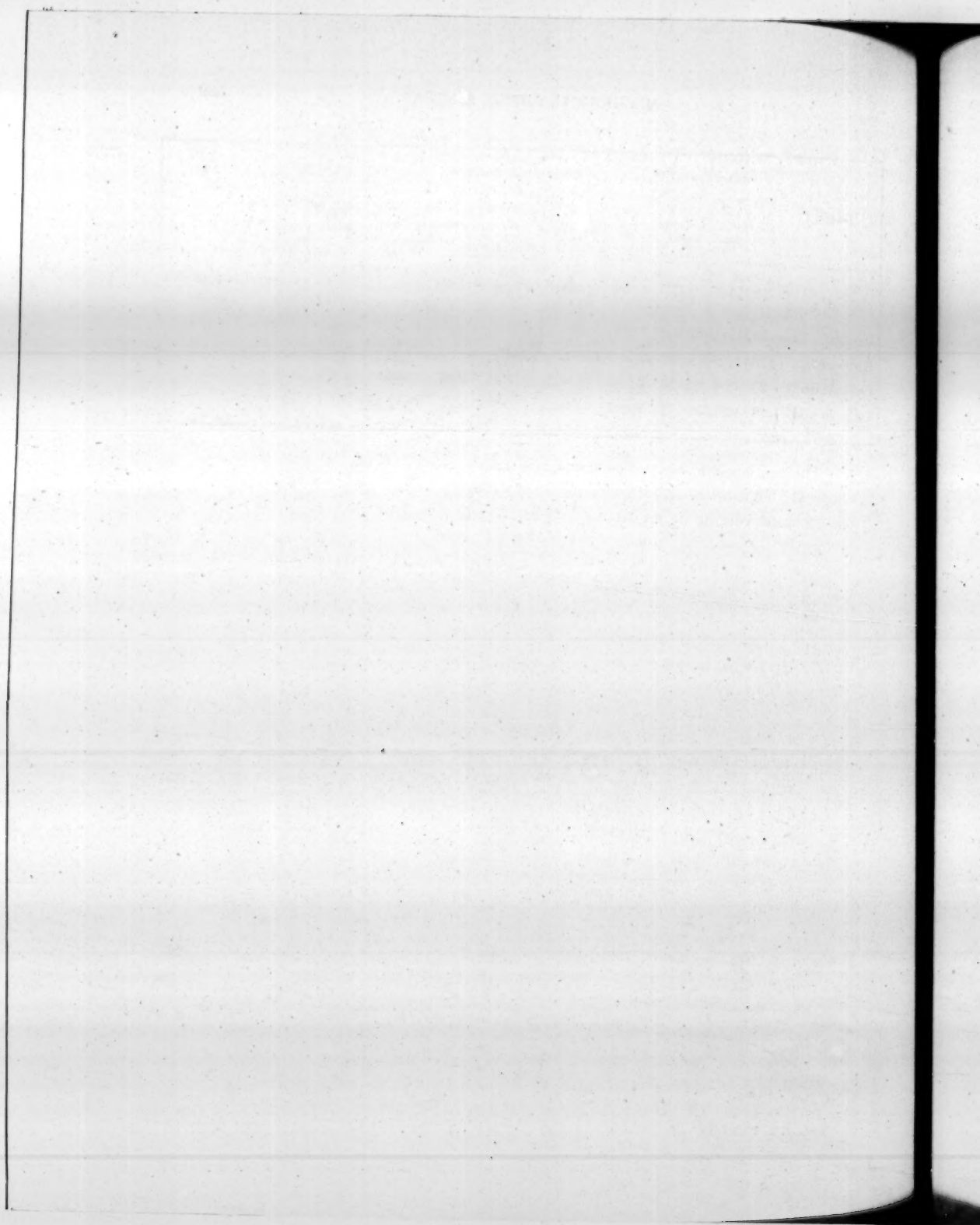
APPENDIX.

NOTE A (to Article 58). Since this section was read, the theoretical views relative to the relation between heat and temperature contained in it and the previous sections of this paper, have received a strong confirmation by the publication by M. REGNAULT of the fact, that he has found the specific heat of air to be sensibly constant at all temperatures from -30° centigrade to $+225^\circ$, and at all pressures from one to ten atmospheres (*Comptes Rendus*, 18th April 1853); so that equal lengths on the scale of the air thermometer represent equal quantities of heat.

NOTE B (to Article 62). Until very recently, there existed no exact experimental determination of the specific heat of any gas. The specific heat of air at constant pressure, as compared with that of water, was calculated theoretically in the previous part of this paper, from JOULE's equivalent and the velocity of sound, and found to be 0.24. This value has since been confirmed very closely by Mr JOULE's experiments, whose mean result was 0.23, and still more exactly by M. REGNAULT's experiments, already referred to, which give the value 0.2379. The following table shews the results of the application of the formulæ of this paper to the specific heats of five different gases at constant pressure, selected from M. REGNAULT's table (*Comptes Rendus*, 18th April), as being those in which the velocity of sound can be computed, and has been determined experimentally. The table shews also a comparison of the calculated and observed velocities of sound. This table appeared originally, in French measures, in the *Philosophical Magazine* for June 1853: the metres are here reduced to feet. K_F , K_V , and K_u , are expressed in feet of fall per centigrade degree. K_w (JOULE's equivalent) = 1389.6.

GASES.	EXPERIMENTAL DATA.			THEORETICAL RESULTS.			VELOCITY OF SOUND AT 0° CENT.		
	PV at 0° C. REGNAULT.	$K_P \div K_V$ REON.	K_P	$K_P - K_V$	K_V	$K_P \div K_V$	By Theory.	By Obser- vation.	Observers.
Air, . . . {	Feet. 26214.4	0.2379	Feet. 330.6	Feet. 96.0	Feet. 234.6	1.4094	Ft. per sec. 1090.4	Ft. per sec. 1090.5 1090.1	BRAYAS & MARTINS. MOLL & VAN BEEK.
Oxygen, . .	23710.4	0.2182	303.2	86.8	216.4	1.4014	1036.4	1042.3	DULONG.
Hydrogen, .	378819.0	3.4045	4731.0	1388.0	3343.0	1.4150	4153.3	4165.1	DULONG.
Carbonic oxide,	27097.8	0.2479	344.5	99.25	245.25	1.4047	1106.8	1107.0	DULONG.
Carbonic acid,	17144.7	0.2169	300.7	64.2	236.5	1.2714	837.55	858.28	DULONG.

The real specific heat of carbonic acid gas is 235.5 feet of fall per centigrade degree. That of the other gases does not differ from the apparent specific heat at constant volume by an amount appreciable in practice.



XLI.—*On Nitric Acid as a Source of the Nitrogen found in Plants.*

By GEORGE WILSON, M.D.

(Read 4th April 1853.)

The source from which plants obtain nitrogen, which is now recognised as one of their most important elements, has, from the first recognition of its importance, been matter of dispute. Latterly, however, chemists and physiologists have pretty unanimously come to the conclusion, that a large (perhaps the largest) part of the nitrogen of vegetables is derived from ammonia; whilst much discussion has been carried on as to the question, Is any part of their nitrogen yielded by nitric acid?

The most able advocate in this country of the claims of ammonia is Dr GREGORY. The most able advocate of the claims of nitric acid is Professor JOHNSTON of Durham, and the opposite conclusions to which accomplished chemists like these have come in reference to the point in dispute, have perplexed botanists, who know not which view to prefer. The extent to which they are pressed by this dilemma, has been so strongly represented to me by Dr BALFOUR, that I have engaged to bring the subject, as I now do, before this Society. I shall sedulously avoid discussing the question in a polemical spirit; and, as the shortest and most satisfactory way of doing justice to the rival views, I shall select Dr GREGORY's clear and concise statement, as representing the opinions of those who deny that nitric acid is part of the food of plants; and then proceed to state what appear to me conclusive proofs that nitric acid does supply plants with nitrogen.

Dr GREGORY writes thus:—"Let us now attend to the nitrogen of plants. This, as already stated, is supplied to wild plants entirely by the air, and, so far as we know, only in the form of ammonia. Some authors have held that nitric acid furnishes nitrogen to plants, and that this acid is formed in the air by thunder-storms, and carried down by the rain. And they point to the occurrence of nitric acid in springs in proof of this. Now it is true that nitric acid is formed in thunder-storms, but in very minute quantity, whereas ammonia is, and must be, present in the air at all times. Indeed there is reason to believe that the nitric acid of storms is produced by the oxidation of the ammonia of the air, as in nitrification, where ammonia is oxidised into nitric acid and water $\text{NH}_3 + \text{O}_2 = \text{NO}_2 + 3\text{HO}$; so that even if nitric acid did yield nitrogen to plants, that nitrogen would be derived from ammonia. This would account, too, for the small amount of nitric acid formed. For if it were produced by the action of electricity on the nitrogen and oxygen of the air, there seems to be no reason why it

should not be formed in very large quantity; while ammonia forms less than one-10,000th of the air, perhaps much less. Nitric acid is only found in springs where decaying organic matter is near them, as in towns, and is formed from the ammonia produced in their decay, by the same process as in nitrification. Besides, while we have no proof that plants decompose nitric acid, which it is certainly possible they may do, we know that many plants, such as tobacco and sunflower, actually produce nitric acid, or, at least, do not destroy that which enters them."*

Thus far Dr GREGORY. I at once concede to him that plants are largely indebted to ammonia for the nitrogen found in them; and in support of the belief that they are also indebted to nitric acid for their nitrogen, I adduce the following proofs.

Firstly, The production of nitric acid in the atmosphere during thunderstorms, is a certain, not a questionable fact; and the scale on which it is produced is such as to necessitate its recognition as a portion of the azotised food of plants. That this should have been questioned is perhaps not strange, for the newly-discovered truth that ammonia is generally present in the air, could scarcely fail to throw into temporary oblivion the equally important truth that nitric acid is generally present there also. The name of the great living chemist LIEBIG is identified with the one discovery, and the name of the great dead chemist CAVENDISH with the other; and we must not grudge that greater interest should be felt by most in the doings of the living philosopher. But assuredly it is not necessary to set the two truths against each other, as if they were mutually incompatible, or in any respect contradictory. On the other hand, I believe that they are complementary, and form an essential and manifest part of that harmonious adjustment which we everywhere perceive guarding plants and animals against imperfect nourishment or decay.

In the year 1781, CAVENDISH addressed himself to the task of answering this question, among others, "Why does the passage of an electric spark through a confined portion of air, cause a diminution in its volume?"† He did not give a categorical reply to this question till 1785, when he published his discovery that a mixture of two measures of nitrogen and five measures of oxygen can be *entirely* converted into nitric acid, by sending a succession of electric sparks through it.‡ He had observed the fact, however, in 1781, in the course of the famous experiments which led to the discovery of the composition of water,—a truth to which I refer, because an impression is prevalent, that the conversion of a mixture of nitrogen and oxygen into nitric acid by the electric spark, can only be effected with great difficulty, whereas the undesired and unintended production of this acid, in trials instituted with a totally different object in view, was the chief

* GREGORY'S *Organic Chemistry*. Third Edition, p. 466.

† *Phil. Trans.*, 1784, p. 119.

‡ *Ibid*, 1785, p. 372.

cause of the delay which attended the announcement that water is not a simple body. CAVENDISH's later experiments were repeated by a Committee of the Royal Society at his own request, and with entire success;* and if any one is slow to repose faith in chemical experiments made in 1785, let me remind him that FARADAY has shewn that every time a friction electric machine is in action, the truth of CAVENDISH's observations may be proved, by no more complex device than the stretching of a piece of paper wetted with solution of potass, across the interval separating two surfaces, between which electric sparks are passing. The potass is quickly changed into nitrate of potass.†

Resting upon these observations of CAVENDISH and FARADAY, I urge the conclusion, that every lightning flash *must* convert a portion of the air into nitric acid; and that in tropical regions where thunder-storms prevail, this acid must be produced largely and almost constantly.

Secondly, As for the proposition that the ammonia of the atmosphere is converted by simple oxidation, as in the process of nitrification at the surface of the earth, into nitric acid, I might leave it unconsidered, for my concern is simply with nitric acid, not with its source. I am quite prepared to admit the probability of atmospheric ammonia undergoing conversion into nitric acid; for although one condition essential to nitrification in the soil, namely, the presence of an alkali or alkaline earth is wanting, yet, from what is known of the intense oxidising power of ozone, we may well believe that when it is developed in the air, as it so certainly and frequently is, it will compel the conversion of ammonia into nitric acid. It will presently, indeed, appear, that, from the recent researches of BARRAL, it is probable that nitric acid is generated in the atmosphere at the expense of ammonia. If this, however, be the case, then we must acknowledge that, in addition to thunder-storms, a force is constantly at work in the air producing nitric acid; and further, that this force is constantly removing from the atmosphere the ammonia on which plants are supposed to be solely dependent for nitrogen.

No data exist from which we can compute, with even an approximation to accuracy, the amount of nitric acid produced by thunder-storms all the world over. It is certainly, however, considerable, as compared with the amount of ammonia in air, and with the amount of nitrogen required by plants.

It is further uncertain how far temperate regions profit by the nitric acid developed by the storms of tropical latitudes; but from the known effects of the winds, and of the diffusive force of gases, in spreading through the atmosphere substances added to one part of it, we cannot doubt that when the heavy rains which so frequently follow thunder-storms, do not at once transfer to the earth the nitric acid which they have produced, it will be conveyed, either free or combined, to immense distances from the spot where it was developed. But upon

* *Phil. Trans.*, 1788, p. 261.

† *Electrical Researches*, vol. i., pp. 90, 91.

this point I do not dwell; for I am content with the alternative conclusion, that the nitric acid of thunder-storms either descends at once to the earth, and feeds the most luxuriant vegetation known to us; or that it is diffused through the entire atmosphere, and is available for the nutrition of the plants of all lands.

Thirdly, Rain-water is often found to contain nitric acid in combination with different bases. The most recent observations on this point with which I am acquainted, are those of M. BARRAL, communicated to the French Academy, and approved by a committee of that body. If BARRAL's results are confirmed, and are not found to be exceptional, they will compel us to acknowledge a much larger proportion of nitric acid, as normally present in the atmosphere, than is generally imagined. His researches were made on the water collected in the rain-gauges of the Observatory of Paris in 1851 and 1852.

The following are his general conclusions:—

“ 1°. During one year, reckoning from July 1, 1851, to June 30, 1852, there fell at Paris a quantity of nitrogen in combination, equal to 20·04 lbs. avoirdupois to the English imperial acre; namely, 11·13 lbs. in the condition of nitric acid, and 8·91 lbs. in the condition of ammonia.*

“ 2°. The quantity of ammonia which fell during that period amounted to 12·29 lbs. to the acre.

“ 3°. The quantity of anhydrous nitric acid which fell during the same period amounted to 41·24 lbs. to the acre.

“ 4°. The quantity of ammonia diminished in the months during which the quantity of nitric acid increased.

“ 5°. The quantity of nitric acid increased whenever the weather became stormy.

“ 6°. During the months only of February, March, April, and June, the quantity of nitrogen in the form of nitric acid, was a little less than the quantity of nitrogen in the form of ammonia.”†

These observations apply to rain-water collected in the neighbourhood of a great city, and do not admit of direct comparison with the purer rain-water of the open country; but they are very remarkable, not merely as shewing that rain brings down nitric acid as well as ammonia, but that, in certain places at least, it contains more nitric acid than ammonia. And although a given weight of ammonia contains three times the amount of nitrogen which the same weight

* There is some mistake in BARRAL's numbers, for the statements in the first paragraph do not agree with those in the second and third; as the numbers, however, for nitrogen are *calculated* from the *observed* quantities of nitric acid and ammonia, the figures representing these are assumed as the correct ones.

If so, the quantity of “nitrogen in combination” is equal to 20·81 lbs. per acre; that of the nitrogen in nitric acid is 10·69 lbs.; and that of the nitrogen in ammonia, 10·12 lbs. These, accordingly, are the numbers which should appear in the first paragraph of BARRAL's conclusions.

† *Comptes Rendus* pour 27 Septembre 1852, p. 431.

of nitric acid does; yet, as in the numbers I have quoted, the weight of acid exceeds that of alkali three and a-half times, it appears that, so far as we have quantitative observations on the matter to refer to, a larger amount of nitrogen is offered to plants in rain-water, in the form of nitric acid, than in that of ammonia.

I have separated the question of the occurrence of nitric acid in rain-water, from that of its development in the atmosphere by oxidation, and by electricity; because it is not certain that the whole of the nitrates found in rain-water have been produced by a process like that of nitrification, or by the action of lightning-discharges on the air. Since rain-water is found to contain common salt, lime, magnesia, and the like, which have been raised into the atmosphere from the earth, or from the bodies of water at its surface, we cannot refuse to credit that nitre may be elevated in the same way. I might go further, for attention has long been directed to the fact, that there is a marked loss during the evaporation of solutions of common nitre, in consequence of that salt, although not volatile in the dry state, undergoing volatilisation along with the vapour of water. This is a secondary point, but it is important, as shewing that, apart altogether from oxidation, and from thunder-storms, there is a source from which the atmosphere everywhere may receive compounds of nitric acid.

Fourthly, It has been known for more than a century, that many springs contain nitrates.

Fifthly, It is now universally admitted, that wherever nitrogenous vegetable or animal matter is exposed to the air along with alkaline bases, ammonia is developed, and then oxidised into nitric acid, which combines with the bases. Now, those conditions are extensively realised all over the globe, both in cultivated and uncultivated tracts of land; and in the warmer regions of the earth, where decomposition proceeds with greatest rapidity, the production of nitre in the soil is constant and immense. India alone furnishes Great Britain with all the nitre needed for her gunpowder.

Sixthly, The most marked nitrous districts of India are celebrated for their fertility, provided a due supply of water is furnished to them.

Seventhly, The alkaline nitrates dissolved in water, and not employed in too strong solutions, have been found greatly to quicken the growth of plants; and the nitrate of soda which, from its cheapness, is the most accessible, is daily coming into greater use among our farmers. In the current number of the Journal of the Royal Agricultural Society,* will be found the last of a series of papers on this subject, in which the virtues of nitrate of soda in increasing the amount of wheat yielded by a field manured with it, are placed by Mr PUSEY above those of ammonia.

It has been asserted, indeed, that alkaline nitrates are serviceable to plants only by furnishing them with alkalies; but I know not by what arguments it is proposed to defend this opinion. It is at variance with the experience of farmers, who find nitrate of soda, as Mr PUSEY reports,† a powerful fertiliser where common

* Vol. xiii., Part ii., p. 366.

† *Ibid.* p. 349.

salt is of no avail. But it is needless to enlarge upon this, for even if it were conceded that soda is the more important constituent of nitrate of soda, considered as a fertiliser, it is manifest that it must make a difference to a plant whether soda be supplied to it combined with carbonic, hydrochloric, sulphuric, or nitric acid; and that in the case of nitrate of soda, the plant must in some way dispose of the nitric acid before it can avail itself of the soda; so that the question must be answered, What becomes of the nitric acid which enters plants?

To this, one reply is offered in the quotation which I commenced by reading. The presence of nitrates in tobacco, sunflower, and certain other plants, is thought to shew that if they do not even possess the power of producing nitric acid, they at least cannot decompose it. But surely this is proving too much. For if the presence of undecomposed nitric acid in a plant shews that it cannot decompose that acid, then the presence of ammonia shews that it cannot decompose ammonia, and the presence of undecomposed sulphuric acid shews that it cannot decompose this acid; and for the same reason, as plants all contain undecomposed chlorides, carbonates, water, and carbonic acid, it should be held that they can decompose none of these. In short, it should be contended that a plant can decompose nothing, and that its existence is a chemical contradiction. If we refuse to draw this conclusion in the case of the other oxides and acids which are found in plants, we must extend our refusal to nitric acid, which is circumstanced exactly as the others are.

As for the opinion that plants may *produce* nitric acid, it is quite possible that they can, although for reasons to be presently mentioned it does not seem likely that they generally do; but it would be unwise to speak confidently on the matter. The only conclusion certainly deducible from the presence of nitrates in plants is, that at least nitric acid does not act injuriously on them.

Thus far, then, it has been I think satisfactorily shewn—

Firstly, That nitrates are largely offered to plants, both as they grow wild and as they are artificially cultivated.

Secondly, That plants do not refuse the nitrates thus offered them.

Thirdly, That the nitrates which enter plants do not, if properly diluted, do injury to any class of them; whilst,

Fourthly, They largely promote the growth of many of the most important among them.

It remains to inquire, Can plants decompose nitric acid, and avail themselves of its nitrogen? I can offer no direct or demonstrative proof that plants possess the power of effecting this decomposition. Direct proof is not to be had in the matter, but the following powerful considerations may be urged in support of the belief that plants can decompose nitric acid.

From the moment of the discovery that water is the oxide of hydrogen, chemists perceived that the great characteristic function of a living plant, considered as a piece of chemical apparatus, was to deprive oxides of their oxygen,

or to deoxidise them. The earliest teachers of this doctrine, CAVENDISH, WATT, MEUSNIER, and LAVOISIER, supposed this deoxidising power to be chiefly expended upon water. At a later period, when the fact that carbonic acid is an oxide of carbon was discovered, and PRIESTLEY's experiments on the conversion of fixed air into free oxygen, by the green leaves of plants in the presence of sunshine, were recalled, the deoxidising powers of a plant were supposed to be mainly expended on carbonic acid. At present, we should decline to say whether this acid or water was most the subject of deoxidation in plants; and we should add to those oxides, sulphuric acid, as constantly undergoing separation into its elements. To such a conclusion we are driven by the fact, that whilst unoxidised sulphur is found in many of the constituents of plants, sulphates are the only compounds of sulphur which are found entering them.

Whatever else, however, is doubtful, this is certain, and is acknowledged by chemists of every school, viz., that a plant is like a blast-furnace, which the sun kindles every day into full action; and that no oxide can pass through such an apparatus, without risking the loss of all its oxygen. With what consistency, then, can it be contended, that water, carbonic acid, and sulphuric acid, cannot pass through a plant in the presence of sunshine, without being deprived in whole or in part of their oxygen, but that the much more easily deoxidised nitric acid, in the same circumstances, will not suffer deoxidation? It might as well be affirmed that a blast-furnace may be competent to reduce the refractory oxide of iron, and yet be incompetent to reduce the easily reducible oxide of lead.

No one I think will deny, that out of a plant, sulphates are deprived of oxygen with much more difficulty than nitrates are; if, however, the deoxidising force at work within a plant can deprive sulphates of their oxygen, *à fortiori* it can deprive nitrates of their oxygen, and we must concede their deoxidability and deoxidation.

But further, the alkaline nitrates which are the medium of the introduction of nitric acid into plants, will certainly within them separate more or less completely into acid and alkali, and let the former become free. Those who contend that nitrate of soda profits plants only in so far as it contains soda, imply by this statement that the nitric acid is set free from the soda, and in *some* way disposed of. All chemists, moreover, will acknowledge that the large amount of fixed alkaline bases found present in every plant in union with organic acids, compels us, whatever theory we hold, to look upon these acids developed within the plant, as having taken the place of the inorganic or mineral acids which accompanied the bases into its structure. Nitric acid must therefore be often set free within vegetable organisms; and when set free, must more rapidly than any uncombined inorganic oxide which is present in plants, suffer instant deoxidation. This proposition, I think, needs no proof. Uncombined carbonic or sulphuric acid, cannot be deoxidised by any known artificial process, so as to separate its oxygen as free gas. Water can be made to yield free oxygen only by a powerful voltaic current,—by an intense

white heat, assisted by platina,—or by chlorine and its congeners, with their affinities for hydrogen exalted by sunshine; but nitric acid is the frailest of oxides. It not only parts with oxygen to the immense majority of metals, and of metallic and organic compounds, but the simple application of heat deoxidises it; and sunlight, which so greatly intensifies the inherent deoxidising power of a plant, can, without the co-operation of its complex organic apparatus, compel nitric acid to undergo deoxidation.

If, therefore, sunlight *alone* can deoxidise nitric acid, sunlight, co-operating with a powerful deoxidising apparatus, will not be less efficacious; and those chemists who declare that a plant can deoxidise water, carbonic acid, and sulphuric acid, but cannot deoxidise nitric acid, are uttering the paradox, that the more easy the decomposition of an oxide is, the more difficult does a plant find it to be to decompose it; so that if it be exceedingly susceptible of deoxidation, then the plant, whose greatest chemical power is a deoxidising one, cannot deoxidise it at all.

No one, I think, would articulately defend such a doctrine. The opposite conclusion is surely the just one, that if nitric acid be conveyed into plants, it will be reduced by loss of oxygen finally to the condition of nitrogen, and as such be as available for the production of azotised vegetable compounds as the nitrogen of ammonia.

Teachers of chemistry appear to be reluctant to admit two sources of nitrogen for plants, because it complicates their statements, and multiplies their formulæ; but the partial representations of truth, to which all teachers are compelled, however catholic in spirit, can never justify the expression of one-sided views, as the counterpart of the multiform unity of Nature. Those, moreover, who have been accustomed to trace back all azotised vegetable compounds to ammonia, need only postulate that nitric acid having been deoxidised into nitrogen, that element unites with hydrogen to form ammonia before any organic compound is developed; and thereafter they may carry out the ammonia theory as before. Such a conversion of nitric acid into ammonia is not hypothetical, for it can be readily effected by diluting the acid largely with water, and dissolving zinc in it.

It would more consist with the modesty of true science, to be less dogmatic than we generally are on the phenomena which occur within the inscrutable recesses of a living plant; and to admit the probability of its being able to employ as food various azotised, as well as other compounds. If, however, we are required to reduce to its simplest chemical expression the conclusion which our present science warrants regarding the inorganic origin of the nitrogen so essential to plants, we must not say that only ammonia, or only nitric acid, is its source, but that both are; or, in a word, that the chief mineral or inorganic representative and parent of the nitrogenous constituents of plants and animals is the Nitrate of Ammonia.

**XLII.—*Some Observations on Fish, in relation to Diet.* By JOHN DAVY, M.D.,
F.R.S. Lond. & Edin., Inspector-General of Army Hospitals, &c.**

(Read 18th April 1853.)

What are the nutritive qualities of fish, compared with other kinds of animal food? Do different species of fish differ materially in degree in nutritive power? Have fish, as food, any peculiar or special properties? These are questions, amongst many others, which may be asked, but which, in the present state of our knowledge, I apprehend it would be difficult to answer in a manner at all satisfactory.

On the present occasion, I shall attempt little more than an opening of the inquiry, and that directed to a few points,—chiefly those alluded to in the foregoing queries.

1. Of the Nutritive Power of Fish.

The proposition probably will be admitted, that the nutritive power of all the ordinary articles of animal food, at least of those composed principally of muscular fibre, or of muscle and fat, to whatever class belonging, is approximately denoted by their several specific gravities, and by the amount of solid matter which each contains, as determined by thorough drying, or the expulsion of the aqueous part, at a temperature such as that of boiling water, not sufficiently high to effect any well-marked chemical change.

In the trials I have made, founded on this proposition, the specific gravity has been ascertained in the ordinary hydrostatical way;—the portions subjected to trial, in the instance of fish, have been taken from the thicker part of the back, freed from skin and bone, composed chiefly of muscle. And the same or similar portions have been used for the purpose of determining their solid contents, dried in platina or glass capsules of known weight, and exposed to the process of drying till they ceased to diminish in weight.

The trials on the other articles of diet, made for the sake of comparison, both as regards specific gravity (excepting the liquids), and the abstraction of the hygroscopic water, or water capable of being dissipated by the degree of temperature mentioned, have been conducted in a similar manner.

The balance used was one of great delicacy, at home, or a small portable one, when from home, of less delicacy, yet turning readily with one-tenth of a grain.

The results obtained are given in the following tables. In the first, on some different species of fish; in the second, on some other articles of animal food.

I have thought it right, whenever it was in my power, to notice not only the time when the fish were taken, but also the place where they were procured,—not

always so precise as I could wish,—as both season and locality may have an influence on their quality individually. When the place mentioned is inland, it must be understood, that, in the instance of sea-fish, they were from the nearest seaport.

TABLE I.

Species of Fish.	Specific Gravity.	Solid Matter, per cent.	Place where got, and Time.
Turbot, <i>Rhombus maximus</i> ,	1062	20.3	March. Liverpool.
Brill, <i>R. vulgaris</i> ,	1061	20.2	October. Penzance.
Haddock, <i>Gadus aeglefinus</i> ,	1056	20.2	August. Ambleside.
Hake, <i>G. merluccius</i> ,	1054	17.4	October. Penzance.
Pollack, <i>G. pollachius</i> ,	1060	19.3	October. Penzance.
Whiting, <i>Merlangus vulgaris</i> ,	1062	21.5	March. Chester.
Common Cod, <i>Morhua vulgaris</i> ,	1059	19.2	April. Ambleside.
Red Gurnard, <i>Trigla cuculus</i> ,	1069	23.6	October. Penzance.
Dory, <i>Zeus faber</i> ,	1070	22.9	October. Penzance.
Mackerel, <i>Scomber scombrus</i> ,	1043	37.9	October. Penzance.
Sole, <i>Solea vulgaris</i> ,	1065	23.0	February. Ambleside.
Do. do.,	1064	21.1	February. Ambleside.
Thornback, <i>Raia clavata</i> ,	1061	22.2	October. Penzance.
Salmon, <i>Salmo salar</i> ,	1071	29.4	{ March. River Boyne, Ireland. Fresh run from the sea.
Sea-Trout, <i>S. eriox</i> ,	...	41.2	June. Ambleside.
Charr, <i>S. umbla</i> ,	1056	22.2	November. Windermere.
Trout, <i>S. fario</i> ,	1053	22.5	{ March. Lough Corrib, Ireland, Weight about $\frac{1}{4}$ lb., in good condition.
Do. do.,	1050	18.7	{ October. River Brathay. A small fish of about 2 oz.
Smelt, <i>S. eperlanus</i> ,	1060	19.3	March. Liverpool.
Eel, <i>Anguilla latirostris</i> ,	1034	33.6	June. Ambleside.

TABLE II.

Kinds of Food.	Specific Gravity.	Solid Matter, per cent.	Place and Time.
Beef, sirloin, ...	1078	26.9	March. Ambleside.
Veal, loin, ...	1076	27.2	November. Ambleside.
Mutton, leg, ...	1069	26.5	November. Ambleside.
Pork, loin, ...	1080	30.5	January. Ambleside.
Pemican, composed of beef and suet, ...	...	86.25	Victualling-yard, Portsmouth.
Common fowl, breast, . .	1075	27.2	November. Ambleside.
Grey Plover, breast, . .	1072	30.1	November. Ambleside.
Cow's milk, new, before the cream had separated, . . }	1031	11.2	November.
White of hen's egg, . .	1044	13.9	
Yolk of the same, . .	1032	45.1	

These results I would wish to have considered merely as I have proposed in introducing them, viz., as approximate ones. Some of them may not be perfectly correct, owing to circumstances of a vitiating kind, especially the time of keeping. Thus, in the case of the whiting, which was brought from Chester, its specific gravity, and its proportion of solid matter may be given a little too high, owing to some loss of moisture before the trials on it were made. Casting the eye over the first table, it will be seen that the range of nutritive power, as denoted by the specific gravity, and the proportion of solid matter, is pretty equable, except in a very few instances, and chiefly those of the salmon and mackerel. The one exhibiting a high specific gravity, with a large proportion of solid matter; the other, a low specific gravity, with a still larger proportion of matter, viz., muscle and oil, and, in consequence of the latter, the inferior specific gravity. A portion of the mackerel, I may remark, merely by drying and pressure between folds of blotting paper, lost 15.52 per cent. of oil. Oil also abounded in the sea-trout and eel, and hence the large amount of residue they afforded.

Comparing *seriatim* the first table with the second, the degree of difference of nutritive power of those articles standing highest in each, appears to be inconsiderable, and not great in the majority of the others, exclusive of the liquids,—hardly in accordance with popular and long received notions.

2. Of the Peculiar Qualities of Fish, as Articles of Diet.

I am not prepared to enter into any minute detail on this important subject, from want of sufficient data.

That fish generally are easy of digestion, excepting such as have oil inter-fused in their muscular tissue, appears to be commonly admitted, as the result of experience,—a result that agrees well with the greater degree of softness of their muscular fibre, comparing it with that either of birds or of the mammalia, such as are used for food.

A more interesting consideration is, whether fish, as a diet, is more conducive to health than the flesh of the animals just mentioned, and especially to the prevention of scrofulous and tubercular disease.

From such information as I have been able to collect, I am disposed to think that they are. It is well known that fishermen and their families, living principally on fish, are commonly healthy, and may I not say above the average; and I think it is pretty certain, that they are less subject to the diseases referred to than any other class, without exception. At Plymouth, at the Public Dispensary, a good opportunity is afforded of arriving at some positive conclusion—some exact knowledge of the comparative prevalency of these diseases in the several classes of the community. The able physician of that institution, my friend, Dr COOK-WORTHY, at my request, has had the goodness to consult its records, and from a communication with which he has favoured me, it appears that of 654 cases of

"confirmed phthisis and of hæmoptysis, the probable result of tuberculosis," entered in the register of the Dispensary, 234 males, 376 females, whose ages and occupations are given individually, the small number of four only were of fishermen's families,—one male and three females,—which is in the ratio of one to 163·2; and of watermen "who fish with hook and line, when other work is scarce, generally very poor, and of habits generally by no means temperate or regular," the number, including their families, did not exceed eleven, of whom ten were males, one a female, which is in the ratio of one to 58·8. The entries from which the 654 cases are extracted, Dr COOKWORTHY states, exceed 20,000. He assures me, that had he taken scrofula in all its forms, the result would, he believes, have been more conclusive.

Such a degree of exemption as this return indicates in the instances of fishermen and boatmen, is certainly very remarkable, and deserving of attention, especially considering the prevalency of tubercular consumption, not only in the working classes generally throughout the United Kingdom, but also amongst the regular troops, whether serving at home or abroad, and having an allowance of meat daily, but rarely tasting fish.*

If the exemption be mainly owing to diet, and that a fish diet, it may be presumed that there enters into the composition of fish, some element not common to other kinds of food, whether animal or vegetable. This I believe is the case, and that the peculiar element is iodine.

I may briefly mention, that in every instance in which I have sought for this substance in sea-fish, I have found distinct traces of it, and also, though not so strongly marked, in the migratory fish, but not in fresh-water fish. The trials I have hitherto made have been limited to the following, viz., the Red Gurnet, Mackerel, Haddock, Common Cod, Whiting, Sole, Ling, Herring, Pilchard, Salmon, Sea-Trout, Smelt, and Trout. In each instance, from about a quarter a pound to a pound of fish was dried and charred, lixiviated, and reduced to ashes, which were again washed. From the sea-fish, the washings of the charcoal afforded a good deal of saline matter on evaporation; the washings of the ash less. The saline matter from both consisted principally of common salt, had a pretty strong alkaline reaction, and with starch and aqua regia, afforded, by the blue hue produced, clear proof of the presence of iodine. In the instance of the fresh-run Salmon, Sea-Trout, and Smelt, a slight trace of iodine was thus detected; in the spent Salmon descending to the sea, only a just perceptible trace of it was observable, and not a trace of it either in the Parr or in the Trout.

That iodine should enter into the composition of sea-fish, is no more perhaps than might be expected, considering that it forms a part of so many of the inhabi-

* In 1205 fatal cases, not selected, in which the lungs were examined at the General Hospital, Fort Pitt, Chatham, tubercles were found to exist in 734 (61·7 per cent.) See the author's "Notes on the Ionian Islands and Malta," vol. ii., p. 312, for details.

tants of the sea on which fish feed ;—to mention only what I have ascertained myself,—in the common Shrimp I have detected it in an unmistakeable manner, and also in the Lobster and Crab ; and likewise in the common Cockle, Mussel, and Oyster.

The medicinal effects of cod-liver oil, in mitigating if not in curing pulmonary consumption, appear to be well established. And as this oil contains iodine, the analogy seems to strengthen the inference that sea-fish generally may be alike beneficial.

Should further inquiry confirm this conclusion, the practical application of it is obvious ; and fortunately, should fish ever come into greater request as articles of food, the facility with which they may be preserved, even without salt, by thorough drying, would be much in favour of their use. I lay stress on thorough drying, as that seems essential,—for preservation, I believe even hygroscopic water should be excluded. Even in the instance of those articles of food which can be preserved in their ordinary dry state, the expulsion of this water would be advantageous under certain circumstances, were it merely on account of diminution of weight. Thus, referring to the second table, it will be seen that the Pemican, carefully prepared in the Portsmouth Victualling Office, lost by thorough drying 13·75 per cent., so much being the water it contained in a hygroscopic state,—a lightening of weight that, to the Arctic land explorer, could not fail to be welcome and useful.

The inference regarding the salutary effects of fish depending on the presence of iodine, in the prevention of tubercular disease, might be extended to some other diseases, especially to that formidable malady goitre, the mitigation or cure of which has, in so many instances, been effected by iodine ; and which, so far as I am aware, is entirely unknown amongst the inhabitants of seaports and sea-coasts, who, from their situation, cannot fail to make more or less use of fish.

Amongst the many questions that many be asked in addition to those I have proposed, I shall notice one more only, and that in conclusion. It is, whether the different parts of the same fish are likely to be equally beneficial in the manner inferred,—the beneficial effect, it is presumed, depending on the presence of iodine. From the few experiments I have yet made, I am led to infer, reasoning as before, that the effects of different parts will not be the same, inasmuch as their inorganic elements are not the same. I may instance liver, muscle, and roe or milt. In the ash of the liver and muscle of sea-fish, I have always found a large proportion of saline matter, common salt abounding, with a minute portion of iodine,—rather more in the liver than in the muscle,—and free alkali, or alkali in a state to occasion an alkaline reaction, as denoted by test paper ; whilst in their roe and milt I have detected very little saline matter, no trace of iodine, or of free alkali ; on

the contrary, a free acid, the phosphoric, analogous to what occurs in the ash of the yolk of the domestic fowl,—and in consequence of which, the complete incineration of the roe of the fish and its milt, like that of the yolk of the egg, is very difficult.

The same conclusion, on the same ground, viz., the absence of iodine, is applicable to fresh-water fish,—a conclusion that can hardly be tested by experience, nor is it of practical importance, since fish of this kind enters so sparingly into the ordinary diet of the people.

LESKETH HOW, AMBLESIDE,
April 14, 1853.

P.S.—I have mentioned briefly the test employed to detect iodine. To prevent obscurity, may I be permitted to add a few particulars relative to the mode of proceeding? On a portion of starch in fine powder, that is, in its granular state, aqua regia is poured, or about equal parts of nitric and muriatic acid, in a platina capsule, and then well mixed, using a glass rod. The salt to be tested, either in solution or solid, is then added. The blue tint due to the presence of iodine is immediately produced, if any of this substance, or a sufficiency of it to take effect, be present. The delicacy of this test is, I believe, well known. I have by means of it detected iodine, when one-tenth of a grain of the iodide of potassium was dissolved in 16775 grains of water. Relative to this method, I may further remark, that by well mixing the acid and starch, not only is the starch reduced to a gelatinous state favourable for being acted on by the iodine as liberated, by the action of the chlorine, but also that the excess of chlorine is, to a great extent, got rid of. The platina capsule has appeared preferable to one of glass, as shewing the effect of colour by reflected light more readily and distinctly; and also, I am disposed to think, from some peculiar influence which the metal exercises, favouring the combination of the starch and iodine, similar, it may be, to that of spongy platinum, in effecting the union of oxygen and hydrogen.

In seeking for iodine in animal substances by incineration, it may be well to keep in mind, that, experimentally considered, the liability to error lies in underrating, rather than in overrating the result by the methods employed, and that mainly in consequence of more or less of loss of iodine being sustained in the process of combustion, incineration, and evaporation used. To illustrate this by a simple experiment, I may mention that a portion of water, equivalent to about 1525 grains, in which were dissolved 10 grains of common salt, and .09 grain of iodide of potassium, was quickly evaporated to dryness by boiling. Previously, the iodine could be detected in the mixture by the test I have used; but not afterwards, when the

residual salt was dissolved in the same quantity of water; proving how there had been a loss of the iodine in the operation of boiling; a loss chemists are familiar with, of substances in themselves not volatile, carried off suspended in aqueous vapour.

In stating the comparative exemption of fishermen and their families from pulmonary consumption, as indicated by the Plymouth Dispensary return, I have not given the total number of this class of persons. This deficiency I am now able to supply. From information which I have received, for which I am indebted to the Registrar-General, it would appear, that of the total male population of Plymouth (24,605), the number of fishermen is 726, exclusive of 37 pilots. This large proportional number renders the fact of their exemption the more remarkable, and especially comparing them with a class of the population, altogether different in their habits, and, it may be presumed in their diet, using fish only occasionally when abundant and cheap,—these are the cordwainers or shoemakers, whose number altogether (males) is 608. Now, on consulting the Dispensary return, I find, that the total number of this class that have died of the disease under consideration, has been 37, viz., 19 males and 18 females!

Reflecting on the fact, that iodine has been detected in all the trials I have hitherto made on sea-fish, it seemed probable that guano, considering its origin, would not be destitute of this substance; and the result of experiments has been confirmatory; using the test-method noticed above, a distinct indication of its presence was obtained, both in the instance of the Peruvian and African guano, the only two I have yet tried.

Fig. 15.

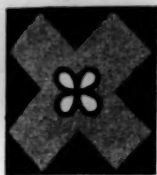


Fig. 16.



Fig. 17.



Fig. 18.



Fig. 20.

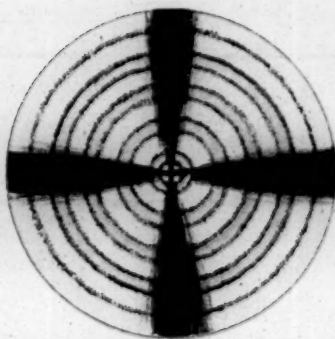


Fig. 19.

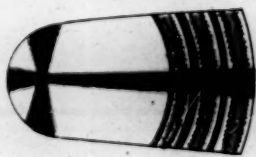


Fig. 21.

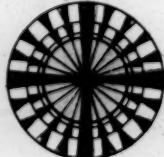


Fig. 22.



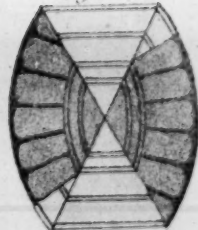
Fig. 23.



Fig. 24.



Fig. 25.



W. H. Lizards Chromo Litho

Fig. 1.

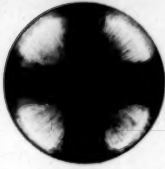


Fig. 2.

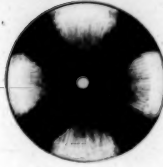


Fig. 3.



Fig. 4.

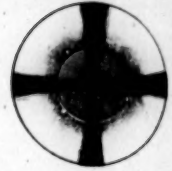


Fig. 7.

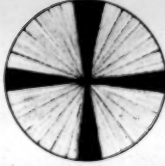


Fig. 8.

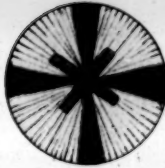


Fig. 9.

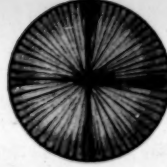


Fig. 10.

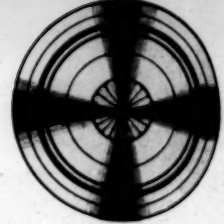


Fig. 6.



Fig. 13.

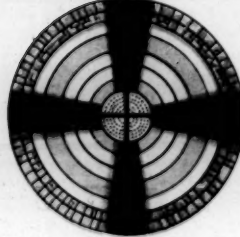


Fig. 11.

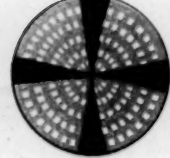


Fig. 5.



Fig. 14.

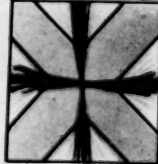


Fig. 12.



W. R. Leese Chromo Litho.

XLIII.—*On Circular Crystals.* By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S.,
V.P.R.S. Edin., and Associate of the Institute of France. (With Two Plates.)

(Read 21st March 1853.)

In 1836, Mr FOX TALBOT communicated to the Royal Society a paper "On the Optical Phenomena of certain Crystals" which he obtained by dissolving a crystal of Borax in a drop of somewhat diluted Phosphoric acid. When the acid and the salt are in proper proportions, "the field of view of the microscope is seen covered with minute circular spots, each of which is like a tuft of silk radiating from a centre, and is composed of a close assemblage of delicate acicular crystals forming a star." Among these crystals are seen interspersed "a number of circular transparent bodies, which are tufts or stars of acicular crystals, in such close assemblage as to be in optical contact with each other, and to produce the appearance of a single individual."

When the field of the polarising microscope is dark, "the little circles become luminous, and we see upon each of them a well-defined and dark cross, dividing the crystal into four equal parts." With a high power, Mr TALBOT observed upon each circle one or more coloured rings arranged concentrically; the innermost, which was deeply coloured or black, enclosing a central space of white light, traversed by the black cross already mentioned. "The general appearance," he adds, "resembles the Fig. 98* in BREWSTER's Treatise on Optics, which is a representation of the rings in uniaxal crystals."

About twenty years† before the publication of Mr TALBOT's paper, I had produced minute circular crystals in *Oil of Mace*,—in a mixture of *Oil of Mace* and *Rosin*, and also in *Tallow*, when these substances were melted between two plates of glass, and cooled under pressure. The crystals thus produced were not themselves visible in the microscope, but, in polarised light, they exhibited their existence and their structure, in the formation of four luminous sectors traversed by a black cross.

When we look through a plate of *Oil of Mace* properly prepared at a bright and small disc of light, the light is generally surrounded with a single halo, though sometimes with two. When the disc of light is polarised and subsequently analysed, the halo is divided into four luminous sectors, separated by a rectangular black cross, which, from the divergency of its branches, had the appearance of four dark sectors. The arms of the black cross were always parallel

* Fig. 120 in the New Edition, just published.

† *Phil. Trans.*, 1815, p. 49, 38; and 1816, p. 97.

and perpendicular to the plane of primitive polarisation, and the disc of bright light disappeared at their point of intersection. In the opposite position of the analyser, the luminous disc appeared at the point of intersection, and the two luminous sectors that were horizontal were brighter than the two vertical ones. These phenomena are shewn in Figs. 1 and 2, Plate XVI.

In some cases, the circular space enclosing the sectors was very small, and in others large, and frequently when two halos were produced, there were two sets of luminous sectors, separated by an interval equal to that between the halos.

It is very obvious that the halos were produced by the crystals of the *Oil of Mace*, the smaller halo by the larger crystals, and the larger halo by the smaller crystals existing among the larger ones. In order to explain the luminous sectors, I inferred that each halo was composed of two, the one lying above the other, and having every alternate sector polarised in opposite planes; or, in other words, that these two halos were the two images formed by the double refraction of the elementary crystals, and were oppositely polarised, as all such images are. But though this inference was correct, as I afterwards proved, yet I could not see with the microscope the actual form of the circular crystals by which the double refraction and polarisation were produced.

After the publication of Mr TALBOT's paper, I repeated the experiment with oil of mace, and having adopted different methods of cooling it under pressure, I soon discovered with the microscope, and by the aid of polarising films when the microscope could not alone detect the structure, that the phenomena which I have described were produced by circular crystals varying from invisibility to the 200th or 300th of an inch in diameter, and exhibiting, when of this size, distinct and beautiful sectors in polarised light.

Having thus discovered a method* of distinguishing true quaquaversus polarisation, or that which is produced by invisible crystalline particles with their axes lying in all directions, from that *apparent* quaquaversus polarisation which is produced by the same class of particles combined in circular crystals, I was anxious to prosecute the subject of circular crystallisation, by examining a great number of doubly-refracting substances.

With this view I received from Mr TALBOT the preparation of Borax and Phosphoric acid which he had found to give the best circular crystals, and from Dr DOWLER of Richmond a quantity of the *Lithoxanthate of Ammonia*, which yields circular crystals with more certainty and less trouble than the preparation of

* This method consists in placing a film of selenite (sulphate of lime) between the polariser and the substance to be examined. If the polarising structure is produced by circular crystals, it will appear covered with spots, or minute sectors, of two different colours, the one being a tint a little lower, and the other a tint a little higher, than that of the selenite. The higher tint is the sum of the tints of the two substances, and the lower their difference, the tint of the selenite being increased by that of two of the sectors, and diminished by that of the other two.

Borax. I was thus enabled to study the phenomena which they exhibit in their formation, their structure, and their subsequent decomposition.

In the expectation of obtaining a greater variety of structure, and discovering new phenomena, I submitted to examination about 300 doubly-refracting substances, and among these I discovered nearly *seventy* that give circular crystals, about *thirty* of which are *positive*, like *Zircon*, and *forty negative*, like *Calcareous spar*.

In the course of these experiments, which have occupied much of my time, I have observed many new and splendid phenomena, which lay open an extensive field of research, and promise to throw much light on those abnormal crystallisations which take place under the constraining influences of heat and pressure, and also on their subsequent decomposition and return to their molecular state.

In submitting to the Society an account of these experiments, I shall begin with the *Lithoxanthate of Ammonia*, as it exhibits a greater variety of phenomena, and is more easily converted into circular crystals than any other salts with which I am acquainted.

1. *Lithoxanthate of Ammonia*.—This substance, under ordinary circumstances, crystallises in minute prisms, often in beautiful dendritic forms, and in spherical groups of crystals in which the prisms are not in optical contact, and yet sufficiently united to exhibit the black cross at the centre of the sphere.

When the circular crystals are produced, and are transparent, they have very different aspects in different specimens. In their simplest form, they are united in a continuous film, each circular crystal exhibiting four luminous sectors separated by a black cross, the arms of which are, of course, always parallel and perpendicular to the plane of primitive polarisation. The light polarised by the sector is the *blue* of the first order, often rising to the *white*, and sometimes to the *yellow*, of the same order.

When we look at a small and bright luminous disc through a film of such crystals, we see a halo, and sometimes two halos, the diameter of the halo diminishing as the circular crystals increase in size. When the film is placed in the polariscope, the halo is converted into four luminous sectors, and into eight when it is double, exactly the same as those produced by oil of mace, and shewn in Figs. 1 and 2.

When the circular crystals are separate, their structure is more complex, and their appearance more beautiful. In one of these, shewn in Fig. 3, I have observed, but only once, the *three* first orders of colours of thin plates, exactly like the uniaxal system of rings in regular crystals; and consequently, the thickness of the spicular crystals which composed them must have increased from the centre outwards, according to the law in NEWTON'S Table of Periodical Colours. This result was so remarkable, that I determined the character of the three

orders of tints, by compensating them with the corresponding tints of plates of selenite.

In other discs the rings 2 and 3 have each the same colour throughout,—the one generally *red* and the other *green*, and having no relation, either to the central tint at 1, or to one another. In some cases the order of colours is completely inverted, as in Fig. 4, where the central tints are a *blue* of the second order, gradually passing through *red* and *yellow* to a brilliant *white* of the first order. In other crystals I have found the central tints *red*, *green*, and *yellow*, of high orders; but in these cases the discs are not regularly formed, and the elementary crystals not wholly in optical contact.

The most perfect circular crystals are those in which the central tints are the *blue* and *white* of the *first* order. This arises from the extreme minuteness of the crystals, which thus form a more uniform disc, and cause the *black cross* to have a degree of sharpness, which it requires a considerable magnifying power to exhibit. In such crystals, the central portion is surrounded with a black and narrow ring, beyond which there is another annulus of sectors, sometimes *white* like the inner ones. This is again terminated by a black circle, beyond which is a third series of sectors, sometimes *white* and sometimes a *blue* of the first order. This structure is shewn in Fig. 5, where the black cross starts into different breadths, in passing from one set of sectors to the others,—an effect which is produced by an inferior degree of optical contact in the elementary crystals of the outer sectors.

An interesting structure is shewn in Fig. 6, where all the tints are *white*, the central ones terminating in a dark circle, beyond which are four large sectors, whose tint is the *bluish white* of the first order, lower than the central tint. Each of these sectors, however, is divided into four portions by very faint circular lines, which scarcely depolarise the incident light, the tint being there a minimum, and increasing to the middle point between them.

In Figs. 7 and 8, we have represented structures consisting of crystals, shooting out, as it were, from the centre, and all of a *golden-yellow* colour. In Fig. 7, the black cross is seen, but in Fig. 8 there is such imperfection of contact between all the radial crystals, that the darkness of those under the black cross is scarcely visible.

In discs like Fig. 7, a very singular effect is sometimes produced, as shewn in Fig. 9, where the black cross is so divergent and wide, that the golden-coloured crystals half-way between its branches, have the appearance of a yellow rectangular cross.

Under favourable circumstances, the discs assume a very interesting and complex appearance, as shewn in Fig. 10. Beyond the central golden-yellow radiations is a broad annulus of *pale blue* of the first order, divided by a faint, dark, and narrow band, scarcely luminous. This annulus is surrounded by a sharp and broad line, perfectly black, which is succeeded by a similar line separated from

the other by a faint line of light, which, in some crystals, reaches the *yellow* of the first order. Beyond this is another annulus of pale blue light, divided, like the first, by a faint line. In some discs this annulus is divided into three, by two faint bands. Each sector of this annulus is subdivided by dark radial lines, into four or five spaces, and, sometimes, beyond this there is another annulus similarly divided, the tint of both being a *white* of the first order.

The interesting fact in this description, and which will afterwards occupy our attention, is, that the two sharp black circular lines, or spaces, are *wholly devoid of matter*, and that the interior part of the disc is separated by them from the exterior part.

Among the almost infinite variety of crystallisations which this substance presents to us, I shall describe only another which, though we shall afterwards find it fully developed in other substances, occurs only in circular sectors of 30° or 45° . It is represented in Fig. 11, in its complete state, and consists of a series of concentric circles, composed of crystalline patches, which generally polarise tints not higher than the *yellow* of the first order. Each concentric circle appears at first to be separated from its neighbour, and each crystalline patch from those adjacent to it; but though this is in some crystallisations the case, yet in general, we can observe between the patches, in all directions, crystalline matter so exceedingly attenuated, that its existence is not made visible by its action on polarised light.

2. *Salicine*.—In this substance, whether dissolved in water or in alcohol, I have found the most splendid circular crystallisations. They are generally very large, and their character is *negative*, like the rings in calcareous spar. When the crystals are small, and require a considerable power to be seen, their tint is the palest *blue* of the first order, but when their diameter is between the one-fifth and the one-thirtieth of an inch, and their tints those of the *first* and *second* orders, they form, in the estimation of all who have seen them, one of the finest objects for the polarising microscope.

One of the smaller crystals is shewn in Fig. 12, where the tint of the four sectors is *bluish-white*, while that of the circular rim is absolutely black, arising from the great thinness of the crystals which compose it. That they are transparent crystals and not opaque matter is proved in this, and in all similar cases, by turning round the analyser when the light freely permeates the rim, and has a slightly yellow tinge, being complementary to what NEWTON calls, in his Table of Periodical Colours, the *Beginning of Black*.

A larger disc of *Salicine* is shewn in Fig. 13, where there is a sharp black cross in the centre, surrounded with five or six narrow and concentric *black* rings, which become *white* by turning the analyser; or we shall in future express it, in the white field. Beyond these central sectors, the black cross is wide and divergent.

The whole of this annulus, which forms the greater part of the disc, is composed of crystals radiating from the centre, and of unequal thickness in their breadth, so that we have the luminous sectors not of one colour, as in the Lithoxanthate of Ammonia, but of various tints from *white* of the first to *blue* of the second order. The radiating crystals are sometimes sectors of 10° or 15° , of uniform thickness, and giving the same colour; and hence, the black cross is composed of sectors of different degrees of blackness as they are brought into the plane of primitive polarisation. Beyond this annulus, the disc terminates in a rim, like that of a carriage-wheel, composed of two or more concentric circles, between which the crystals are disposed in radial lines, sometimes not in optical contact, but exhibiting the same colours as those in the larger annulus. In discs of a considerable size, there are seen exceedingly minute and dark circles, about ten or twelve in number, which I have found to be cracks or lines of cleavage, and which are accompanied with short lines of cleavage, passing radially from the one to the other.

In these discs, there is another peculiarity which deserves to be noticed. In the coloured sectors, there are often circular spots and rings, in which the tint descends to *zero*, as if a drop of some solvent had fallen upon the crystal: and there are spots of an opposite kind, where the tint rises from that of the sector to higher tints, an effect probably produced by a particle of the crystal forming around itself, while dissolving, a thicker film, becoming thinner as it recedes from the particle.

In some of the circular discs of *Salicine*, I have found the outer rim as wide as the interior portion, and in this case it polarises a *bluish-white* of the first order; but, what is peculiarly worthy of notice, this rim is subdivided by faint concentric rings of different degrees of darkness, into, sometimes, twelve or fifteen annuli of different degrees of brightness. This seldom takes place in the interior portion of the disc, but when it does occur, and the tints are brilliant, the subdivision of the annulus into a number of concentric circles of different colours is singularly beautiful.

3. *Asparagine*.—The circularly polarising discs which this substance displays, resemble very much those of *Salicine*. They are more varied in their structure, and more beautiful in their tints. The rims of the discs are more highly coloured, and more uniform in their texture; and the concentric tints, whether they are all of different degrees of whiteness, or of higher orders of colours, are so perfectly regular, and so sharply defined, that the observer stands before them in mute admiration, and feels himself unable either to describe or to draw them. There are two peculiarities, however, which deserve to be noticed; the one, the existence of discs in which there is no circularly polarising structure; and the other, of discs exactly resembling, in the succession of black and white narrow rings, the systems

of rings seen round the star Capella, with annular apertures, and drawn by Sir JOHN HERSCHEL.*

4. *Manna*.—This substance gives fine circular crystals, which are negative, whether obtained from fusion or an aqueous solution. The crystals obtained by melting the Manna are the most perfect and beautiful. The intersection of the arms of the black cross is so sharp that it sometimes requires a considerable power to develope it, and the four minute sectors around it. Beyond this the crystals radiate uninterruptedly till they are stopped by meeting with other crystals, and the whole of them are joined together in a hexagonal mosaic pavement. The colours are very bright, varying from the *white* of the first to the *blue* and *green* of the second order, and there is a uniformity in the tints, and consequently in the shading of the black cross, which indicates great equality in the elementary prisms, and in the forces which keep them in optical contact. The discs are seldom found separate, and they have no rims, no annuli, and no concentric cracks.

5. *Disulphate of Mercury*.—This salt, dissolved in nitric acid, gives no circular crystals by rapid cooling; but, when the solution is cooled slowly, it yields positive circular crystallisations of a square form, as shewn in Fig. 14, which undergo interesting variations. The rectangular cross is sometimes wanting, and is, as it were, replaced by black lines, which meet at the centre. These lines are sometimes black in the white field, and are then junction lines where the optical contact is imperfect. The greater number of the crystals in which these lines are more or less perfectly seen are rounded at the angles. Sometimes they are nearly circular, and the tint which they polarise is very little above the *beginning* of black of NEWTON's Table.

When the crystals are thicker, they exhibit a singular variety of forms, of which I have given a specimen in Figs. 15, 16, 17, and 18, the relation of which to Fig. 14, will be easily recognised. The crystals shewn in Figs. 16 and 18 were obtained from a weak solution of the salt, and are very interesting. In the dark field of the microscope, we see only the brilliant golden-yellow border, and it requires a strong light and a very high power to discover, in the black interior of the square, minute specks of light equally diffused over its surface. By a slight turn of the analyser, we perceive the slightly darker diagonal cross shewn in Fig. 16. These squares are often wholly and uniformly filled up with crystals of the same tint as their outline; and occasionally only part of the square is thus occupied. The small and often shapeless crystals (occasionally oval and pear-shaped), which form the outline of the square in Fig. 16, and of the cross in Fig.

* *Treatise on Light*, § 770, Figs. 155, 156, 157, Plate IX.

18, have placed themselves in these positions after the interior crystal has been formed; that is, they are not increments deposited by the solution, but have been formed at a distance from the crystal, and carried to their new position. This is proved by the fact that sometimes a mass of them surround several of the square crystals, while individual ones take their place at random upon the face of the square. When the crystals are deposited from a strong solution, the square ones become almost opaque, and the irregular ones highly coloured, and of exceedingly various shapes. I have not been able to obtain any square crystals of the disulphate of mercury from its solution in muriatic acid.*

6. *Parmeline*.—This substance, dissolved in water, has a tendency to give circular crystals. In alcohol it gives very fine ones, producing, when small, beautiful halos like oil of mace, with blue light in their centre.

7. *Asparagine and Salicine mixed*.—After standing several months, this mixed salt produced small circular crystals, apparently of asparagine. These crystals gave brilliant halos of red and green light, of such a diameter that the individuals were only $\frac{1}{7500}$ th of an inch in diameter. Among these small crystals were placed large circular discs, with curved sectors and black crosses, which gave them the appearance of the corolla of a flower with party-coloured petals.

8. *Palmic Acid*.—This substance, when melted by heat, gives very fine negative circular crystals like those in the hexagonal mosaic of manna. The insulated discs have a rim sometimes divided by broad black bands, where the substance was too thin to polarise the light. When the rim is broad and single, it is composed of narrow luminous sectors, radiating from points in the circumference of the disc. The rims are sometimes of a different colour from the principal sectors, and the latter are often subdivided by numbers of black and equidistant concentric circles.

9. *Nitrate of Uranium*.—This salt gives fine negative circular crystals in water, alcohol, and ether. The crystals formed in the alcoholic solution deliquesced in an instant, forming hemispherical bells, which polarised the light by oblique refraction, giving four luminous sectors, and a black cross very wide at the centre, like the sectors and cross produced by the hemispherical cups of de-

* In making these observations, and on many other occasions, I have felt the great inconvenience of the present, and in general, perhaps the best, arrangement of the compound microscope. High powers being always obtained by object-glasses of short focal length, it is almost impossible, in transparent structures, to develop them, when they consist of lines or parts of different thickness. Vision is destroyed by the refractions and diffractions of the intromitted light. The only remedy for this is to use $\frac{1}{2}$ -inch, or even 1 or 2-inch object-glasses, and obtain the power that is required at the eye-piece, by means of grooved and other lenses of diamond, garnet, &c.

composed glass. After the deliquescence of the crystals, I attempted to make another crop, but having failed, I set the piece of glass aside. In the course of half-an-hour, however, I found it covered with a fine and splendidly-coloured set of circular crystals, which dissolved wholly when placed in castor-oil with the view of preserving them. The light polarised by the bells above mentioned, formed a double ring, *red* on one side, and *green* on the other, with a black space between.

Upon examining the solution in castor-oil, after having stood upwards of four years, I find that circular crystals of three different kinds have been formed, some small and very perfect, with four sectors and no rim; others with broad rims, with quaquaversus polarisation; and a third set in which the structure has been entirely decomposed, and the circular form of the disc preserved.

10. *Palmine*.—This substance melts like tallow into a uniformly luminous film, apparently with quaquaversus polarisation; but upon examining it with a high power in the polarising microscope, it exhibits millions of circular crystals, each bearing its little black cross. These crystals are so minute as to produce splendid halos, which, in the polariscope, give four luminous sectors exactly like those in oil of mace.

11. *Chromic Acid*.—The circular crystal, of this substance, dissolved in water, are of a very peculiar kind. They are negative, and are very imperfectly represented in Fig. 11, where the circular disc is composed of a great number of concentric circles, whose tint is the *blue* of the first order, rising, in some cases, to the *yellow* of the same order. These circles may be described as rippled lines consisting of minute crystals, separated by others still more minute, and incapable of polarising the light. The system of concentric rings is traversed by the usual black cross. This salt gives another kind of crystals, in which are separate concentric rings without the black cross, and consequently with quaquaversus polarisation.

12. *Berberine*.—This salt gives very fine circular crystals which are negative, and form beautiful halos like those in oil of mace. The ordinary crystals often form a number of crystalline rings in contact, each of which contains circular crystals of different sizes, and occasionally prismatic crystals along with them.

13. *Sulphate of Cadmium*.—The sulphuret of cadmium, dissolved in nitric acid, is converted into sulphate, which gives beautiful negative circular crystals, varying from the 800th of an inch to the 3000th. After the sulphuret is melted, and the acid driven off, no crystallisation is seen, but in an hour or two a deliquescence takes place, and the circular crystals gradually appear. There are many of them so small and thin, that they have no action on polarised light.

The solutions of this substance in muriatic and acetic acids gave no circular crystals.

14. *Sulphate of Ammonia and Magnesia*.—When an aqueous solution of this salt is brought into a viscous state by heat, and slowly cooled, very beautiful circular crystals are formed, sometimes large, and sometimes smaller than the 1000th of an inch. Round some of them there is formed a radiant crystalline halo, separated by a black circle from the disc which gives the four luminous sectors. The polarisation of the crystals is *positive*, and they are perfect at the centre, unless when large and badly formed.

15. *Hatchetine, Cacao Butter, White Wax, Tallow, Adipocire*, and all *Soaps* and different kinds of *Fat*, produce circular crystals like *Oil of Mace*, and give the same halos, which, in the polariscope, are divided into four sectors.

16. *Borax in Phosphoric Acid*.—The crystals produced by this combination are those discovered by Mr TALBOT, and have been already described. I mention them again, in order to notice the interesting hemispherical bells which I have observed when an aqueous solution is raised into froth by heat. These bells or bubbles indurate and polarise the light by refraction, as in the case of *nitrate of uranium* already mentioned. They are traversed by the black cross, and exhibit rings of colour, which are *green* at the centre, then *red*, then *green* and *red* again. Some of these bells contained smaller ones within them, in one case no fewer than eight; and in one of them I distinctly observed a crystalline structure, the minute crystals radiating from the apex of the bell.

17. *Mannite*.—This substance gives circular crystals with more facility and certainty than any other which I have examined. When Mannite is melted by heat, it gives beautiful circular crystals. When dissolved in water they are very good. They are not good in alcohol or ether; but in acetic acid the finest circular crystals are formed. The black concentric circles are peculiarly fine, and are, so far as the microscope can shew it, entirely free of matter. In the crystals from acetic acid, the sectors shade off into the arms of the black cross with such perfection, that the circular disc loses its flat appearance, and seems to be composed of *four solid cones*, whose apices meet in the centre. In place of being circular the crystals are sometimes drawn out, as it were, into long cones, as shewn in Fig. 19, rounded at their summits, and having the appearance of solids of that shape. The black cross appears at the summit of the rounded cones, one of its arms, and sometimes two, according to the position of the plane of primitive polarisation, stretching out to the termination of the rounded cone. These cones are often crossed by *two, three, or four* concentric arches, perfectly black. In these elon-

gated crystallisations, the elementary particles are in perfect optical contact, the tint which they produce being a bright white of the *first* order. At this period of their formation, a crystallised and semi-opaque crust is formed above many of them, the opacity arising from an imperfection in the optical contact. This crust sometimes cracks and falls off, leaving the perfect crystal beneath, or when it merely cracks, shewing the perfect crystal through the fissure. These incrustations sometimes occupy the middle of the spaces between the black arches, *m n*, *o p*, &c., and raise the tint to an orange-brown. In a specimen preserved in Canada balsam, the balsam has insinuated itself between the imperfectly-united elementary crystals, and made the crust so transparent, that the crystal beneath it is most distinctly seen, as if through a piece of glass.

In some specimens, the optical contact is so imperfect, that groups of discs have a pale nut-brown semi-transparency, with the concentric black bands finely developed.

In other specimens, we have every degree of transparency, up to absolute opacity. In some discs, the black cross is scarcely seen, and they seem as if they were composed of fine threads of worsted, from the sides of which other finer threads diverge. Such crystals are beautifully white by reflected light, and look as if they were formed of fibres of white satin.

An interesting peculiarity in the larger discs is shewn in Fig. 21, where each successive ring is formed by radiations from the margin of the preceding ring. These radiations or tufts are occasionally separate, as in the figure, but generally in optical contact, so as to form a luminous ring in which the tints are not uniform.

In weak solutions of mannite, the crystallisations are exceedingly delicate, and the light which they polarise scarcely visible.

18. *Oxalurate of Ammonia (pure).*—This salt, to which my attention was called by Professor GREGORY, and which, according to that chemist, is probably identical with the Lithoxanthate of Ammonia, gives very beautiful *negative* circular crystals.

With weak aqueous solutions the discs are small and beautiful, and very much like those from *Lithoxanthate of Ammonia*, the cross sometimes consisting wholly of circular discs, and at other times of a few discs interspersed among dendritic crystallisations.

From strong solutions the discs are often nearly opaque, and round them are formed concentric rings, consisting of marginal radiations, as in Fig. 20, their elements being often in optical contact, and yielding different polarised tints. Occasionally we find discs, sometimes large ones, in which the central circle consists of tints of the *green* of the second order, with a feebly-developed black cross, descending to the *white* of the first order. This is followed by a narrow black concentric space, beyond which the *white* tint reappears, and rises to the *yellow* of the second order, which again descends to *white*, thus completing the *second* ring.

Other three rings follow in succession, the *white* tint rising to the *yellow*, and again falling to its original colour. Each of these five rings have precisely the same tints throughout their circumference, and when a number of such crystals appear in the dark field, they form objects of singular beauty.

In some specimens, the discs have the appearance of cones, as in *Mannite*. They have, in the centre of the black cross, another cross whose arms bisect the sectors, having sometimes a *white*, or *yellow*, or *green* tint. This cross is surrounded with a faint ring, which separates it from large sectors of a bright *pink* colour.

The circular discs are often composed of radial lines of different thicknesses, and in imperfect optical contact. Their tints consequently vary throughout the disc, and have a remarkable appearance. When the crystals are very small, they produce the polarised halos given by oil of mace.

19. *Hippuric Acid*.—This salt gives imperfect discs when melted. With water, it gives good circular crystals, but very fine ones with alcohol. They have a great variety of forms and tints, depending on the strength of the solution; but they differ from other circular crystals in two points. The radial lines are often separated from one another by black spaces of the same breadth as the luminous radial lines, and the whole disc is covered with almost invisible concentric black circles, at equal distances from one another. They are seen most distinctly in the white field. The four central sectors are often surrounded with a ring separated from them by a black space entirely free from matter. In some specimens, the discs consist of eight or ten sectors of uniform thickness and tint, which become black when in the plane of primitive polarisation. In other specimens, the crystallisations are large, irregular, and highly coloured.

Having thus described the phenomena exhibited by some of the more important circular crystals, I shall give a tabular list of the other substances in which I have found the property of giving circular crystallisations, arranging them under the heads of *Positive* and *Negative*, as formerly explained.

Positive Circular Crystals.

Sulphate of ammonia and magnesia.	Sulphate of iron and ammonia.
... red oxide of manganese.	... potash.
Hydrate of potash.	... manganese and ammonia.
Citrate of potash.	... magnesia and ammonia.
Muriate of morphia.	... zinc and potash.
... magnesia.	Disulphate of mercury.
Almond soap.	Mannite.
Starch.	Citrate of ammonia.
Substance in garnet.	Myristic acid.
... mica.	Cuprose sulphate of potash.
Chloride of strontian.	Kreatinine.
Sulphate of cobalt and ammonia.	

Negative Circular Crystals.

Borax in phosphoric acid.
 Lithoxanthate of ammonia.
 Salicine.
 Asparagine.
 Manna.
 Parmeline.
 Palmic acid.
 Palmine.
 Esculine.
 Berberine.
 Cinchonine.
 Theine.
 Oil of mace.
 Cacao butter.
 Hatchetine.
 Animal fat.
 White wax.
 Chrysopleptic acid.
 Succinate of zinc.
 Chromic acid.
 Citric acid.
 Nitrate of uranium.
 ... urea.
 ... brucine.
 ... strychnine.
 Gallic acid.
 Thianuret of ammonia.
 Sulphuret of cadmium.

Sulphate of copper and iron.
 ... zinc.
 ... magnesia.
 ... magnesia of potash.
 ... copper of ammonia.
 ... zinc of ammonia.
 ... zinc.
 Substance in garnet.
 Stearine.*
 Stearic acid.
 Palmitic acid.
 Acetate of strontian.
 ... quinine.
 Chloride of zinc.
 Oxide of uranium.
 Protoxide of nickel.
 Phosphate of nickel.
 Carbonate of nickel.
 Substance in mica.
 Adipocire.
 Margoric acid.
 Ethal.
 Oxalurate of ammonia.
 Kreatine.
 Carbazotate of potash.
 Sulphuret of potassium.
 Hippuric acid.
 Santonine.

To this list of substances which, under certain favourable conditions of crystallisation, after solution or fusion, give circular crystals, the perfection of which depends on causes over which the observer has little control, I may add the following animal substances, in which the circular phenomena are produced, and in which, with one exception, the structure is negative, as the greater number of such structures seem to be.

Hoof of horse, both transverse and vertical.
 ... ass, transverse section.
 Transparent aperture in the wing of the beetle.

Hoof of rhinoceros.
 Horn of rhinoceros, transverse and vertical.
 ... antelope.
 Hairs of animals, sections of.

In examining the crystallisations of the *Chromate* and *Protochloride of Mercury*, and of the *Sulphuret of Bismuth*, I found that they exhibited the hemispherical

* Stearine gives the same polarised halos as oil of mace.

bells already described, in which oblique refraction and the thinness of the film combine to produce beautiful coloured rings, with a black cross.

In other crystals, such as *Muriate* and *Citrate* of *Quinine*, *Codeine*, and *Nitrate* of *Codeine*, I have observed the luminous sectors, and the black cross round the air-bubbles, which are formed after fusion, a phenomenon exactly the same as that which takes place round cavities in diamonds, amber, and other substances.

Having thus described the principal phenomena of circular crystals, I shall now proceed to make a few observations on their formation and decomposition. Circular crystals are abnormal aggregations, which owe their existence to some disturbing cause. The natural tendency of the elementary molecules of the most perfect of them, is to combine with their homologous axes parallel to one another, and to form regular crystals; and it is only when this tendency is counteracted by the quick application of heat or cold, by pressure, or by the nature of the solvent or of the combined ingredients, as in the case of borax and phosphoric acid, that the molecules are constrained to arrange themselves round a centre, not merely in radiating prisms, as in *Wavellite* and some other minerals, but according to laws which could not have been anticipated from any known principles of crystallisation. If, owing to any disturbing cause, two molecules should be deposited with their axes at right angles to each other, or four with their similar poles directed to the same point, this will lead to the formation of a circular disc, which will be of limited thickness, if the crystallisation takes place between two plates of glass pressed together, or to the formation of a spherical crystal, as in the Lithoxanthate of Ammonia, when there is room for its growth in all directions. The disc, or the sphere, might thus increase to a considerable size, if there was only one centre of crystallisation, but as the same causes have been operating all around, the size of the circular crystal is limited by the number of molecules within its sphere, or by its junction with the other discs around it. In this last case, they form a sort of mosaic, in which their shape is not circular, but hexagonal, as in manna, oil of mace, and many other substances.

In the greater number of circular crystallisations, the tints are a minimum at the centre of the disc, and increase outwards,—that is, the molecules form a thinner film at the centre, which increases in thickness towards the circumference; but in other cases the reverse of this takes place, and in the disc represented in Fig. 3, where the tints are those of NEWTON's rings, some cause, which we cannot even conjecture, must have determined the atoms to unite according to the complex law which connects these tints with the thicknesses at which they are produced. A cause of an opposite kind must have given birth to the disc shewn in Fig. 4, where the molecules form a thick film at the centre, which diminishes in thickness from nine to three as the tint passes from the central *blue* to the *white* at the circumference.

It is equally difficult to assign any reason for the production of the concentric bands of a uniform tint, which suddenly pass to another tint belonging to a different order of colours, and produced by a different thickness of material. A circular ring of *green*, for example, will pass *per saltum*, to a *red* of the next order, from a thickness of 9 to a thickness of 18; and this, according to a law which operates at every point of the circumference of the ring. Nor is this phenomenon less remarkable when the transition takes place in the very lowest order of tints, and at the smallest thickness of the film, as shewn in Figs. 6, 10, and 13, where the tint passes in repeated alternations from the pale *blue* to the beginning of *black*, rising to a *maximum* of *blue*, and again descending to the *minimum* of *black*.

The black rings or circles shewn in Figs. 5, 6, 10, and 13, require to be carefully studied, and with the finest microscopes. In most cases they seem to be spaces devoid of crystalline matter; but they have in general another origin. A line often appears perfectly black, when it corresponds with the *violet* of the *second* order, which separates the *indigo* of the same order from the *red* of the *first* order. Another set of lines appear black, from their being the junction lines of crystals not in perfect optical contact. A third set of black circles are produced by the extreme thinness of the substance, which is not capable of polarising the *very black* of NEWTON'S scale, and the existence of which upon the glass plate can be ascertained only by the highest powers of a fine microscope. But though in all these examples there is no breach of continuity in the circular disc, yet there are cases, as in the double black ring in Fig. 10, where *the corresponding space is devoid of all crystalline matter*. The crystallisation of the disc had been completed at the inner margin of the first black ring, and by some repulsive power the molecules in the solution were kept at a distance from the completed disc, and deposited themselves in a scarcely visible ring around the outer margin of the first black ring. The repulsive power again came into play; and another black ring intervened, the molecules being deposited at the same distance as formerly from the last-formed ring. What repulsive power this is, if it is not electrical, and how it operates, if it is electrical, we cannot even conjecture.

Another remarkable peculiarity in circular crystals is shewn in Figs. 11 and 12, where, as in chromic acid, the disc consists of alternations of dark and luminous circles, equidistant from each other. The dark circles are composed of the acid in particles too small to polarise light, and the luminous ones of separate patches of crystalline matter thick enough to give the *blue* and sometimes the *white* of the first order, and separated from one another by matter too thin to polarise light. In some rare cases, the spaces between the circles and between the patches are, like the black rings formerly described, devoid of crystalline matter. The separation of the patches in this case, is no less remarkable than the separation of the luminous circles. In the Adipocire from Paris, the tint of

the patches sometimes reaches the *yellow* of the first order, and its crystallisation has a very singular appearance.

When the molecules of the same body, or those of different bodies, are combined under the influence of disturbing causes, we may reasonably expect that their union will neither be strong nor permanent. When regular crystals are melted by heat, either alone or along with other bodies, their molecules are forced into positions of unstable equilibrium, and the natural tendency of similar poles to unite is aided by every mechanical vibration, and every variation of temperature to which they are exposed. Different kinds of glass, for example, in which earths, alkalies, and metals may have been combined by fusion, are thus completely decomposed by time, and the elementary particles, liberated from their constrained position, resume their place in crystals regularly formed. The specimens of ancient glass found at Nineveh, and in various parts of Italy and Greece, have undergone the most remarkable decomposition, and some of it converted into a sort of indurated mass, which can be broken between the fingers. The character of these decompositions, and the process by which they are effected, I have had occasion to describe in the *Appendix* to Mr LAYARD's new work on Nineveh and Babylon.* The same principles operate in the decomposition of circular crystals, and the same phenomena are exhibited in their restoration to their original state.

In circular crystals the decomposition takes place in different ways. In those from borax and phosphoric acid, which I have had occasion to watch month after month for several years, the decomposition generally begins at the centre, which is dissolved, or occupied by a number of minute prisms, with their axes lying in every direction. These prisms sometimes are arranged in a ring round the centre, and I have seen them like a St Andrew's cross. In other crystals, the decomposition goes on in radial lines or streaks, where the optical contact has not been complete; but in the more perfect crystals it takes place in concentric circles, sometimes double, the colours between each pair of circles being different. Numerous cavities are formed,—pieces of the crystal separate, and irregular crystals are often formed in the solution. Decomposition sometimes takes place without solution: the crystal preserves its form, the black circles are granulated, and the colours wholly disappear. In one of the specimens in my possession, every crystal has vanished, and their elements converted into beautiful prisms, united like a bunch of straw tightened at the middle. Between these groups there are numerous flat crystals, of considerable size, and of a perfectly uniform tint. All these decompositions have been the work of several years; and in the course of one year more there will not be found a vestige of the original crystals.

In *Manna* the transformation of the circular into their component crystals

* Discoveries in the Ruins of Nineveh and Babylon. By AUSTEN H. LAYARD, M.P. Appendix, p. 674-676.

goes on more slowly, and in a more singular manner. It commences at the hexagonal junctions of the discs, all of which become *black* by transmitted, but *white* by reflected light. These minute crystals, which are transparent when separate, diffuse themselves around, as if they had fallen in a shower. The same kind of decomposition goes on in radial lines, and a granular decomposition takes place over the coloured sectors, commencing at their centre, obliterating the black cross, and destroying the tints of all orders.

In *Oil of Mace*, the decomposition is effected in a single night. The area of the disc is filled with drops of fluid and atoms of solid matter which have no action upon light, while an opaque ingredient occupies its circular margin.

In *Palmerine* and some other crystals, the film decays in spots, where the tint descends from that of the film to *zero* in concentric circles, while in other spots the tint rises in similar rings, as if the atoms, liberated from one spot, had been deposited in another.

Such are the details respecting the nature, formation, and decomposition of circular crystals, which I wish to submit to the Society. Lengthened as they are, they are but a brief abstract of the numerous observations, which, during the last ten years, I have made on this class of bodies. Their bearing upon unsettled questions in the molecular philosophy cannot be doubted. If it is in the agency of its ordinary laws that we recognise the beauty and harmony of the material universe, it is in the abnormal phenomena which so often perplex us, that Nature discloses her mysteries and reveals her laws.

ST LEONARD'S COLLEGE, ST ANDREWS,
15th March 1853.

7

PROCEEDINGS
OF THE
STATUTORY GENERAL MEETINGS,
AND
LIST OF MEMBERS ELECTED AT THE ORDINARY MEETINGS,
SINCE DECEMBER 3, 1849 ;
WITH
LIST OF DONATIONS TO THE LIBRARY,
FROM DEC. 3, 1849, TILL APRIL 18, 1853.



PROCEEDINGS, &c.

Monday, November 26, 1849.

At a Statutory General Meeting, Dr CHRISTISON, V.P., in the Chair, the following Office-Bearers were duly elected :—

Sir T. MACDOUGALL BRISBANE, Bart., G.C.B., G.C.H., President.	
Sir D. BREWSTER, K.H.,	} Vice-Presidents.
Very Rev. Principal LEE,	
Right Rev. Bishop TERROT,	
Dr CHRISTISON,	
Dr ALISON,	
Hon. Lord MURRAY,	
Professor FORBES, General Secretary.	
Dr GREGORY,	} Secretaries to the Ordinary Meetings.
Professor SMYTH,	
JOHN RUSSELL, Esq., Treasurer.	
Dr TRAILL, Curator of Library and Instruments.	
JOHN STARK, Esq., Curator of Museum.	

COUNSELLORS.

Sir WM. JARDINE, Bart.	Sir JOHN McNEILL, G.C.B.
Rev. Dr J. ROBERTSON.	JOHN CAY, Esq.
CHARLES MACLAREN, Esq.	Professor KELLAND.
J. T. GIBSON-CRAIG, Esq.	Very Rev. E. B. RAMSAT.
JAMES DALMAHOY, Esq.	Dr J. Y. SIMPSON.
Dr GEORGE WILSON.	JAMES WILSON, Esq.

The following Committee was appointed to audit the Treasurer's accounts :—

J. T. GIBSON-CRAIG, Esq.	JOHN CAY, Esq.	D. SMITH, Esq.
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The Meeting then adjourned.

(Signed) R. CHRISTISON, V.P.

Memorandum.—January 7, 1850.—At the Ordinary Meeting of this date, Sir THOMAS M. BRISBANE, Bart., in the Chair, the Chairman stated that the Council had caused to be prepared a Memorial to the proper authorities, expressive of a desire that the Trigonometrical Survey should now be carried on in Edinburghshire. The Memorial was read, and, on the motion of Dr FLEMING, seconded by Mr SMITH of Jordanhill, was unanimously adopted, and ordered to be transmitted to the Superintendent of the Trigonometrical Survey of Great Britain.

Memorandum.—*February 18, 1850.*—Of this date, the Council reported that, having been requested to nominate a Committee of the Society, to act as Members of a General Committee about to be formed for the furtherance of the Great Exhibition of Manufactures, &c., in London, in 1851, they had named Dr TRAILL, Professor C. PIAZZI SMYTH, and Dr GEORGE WILSON. Thereafter Mr Sheriff CAY'S name was proposed and accepted as an additional Member of Committee.

Memorandum.—*Monday, March 4, 1850.*—At the Ordinary Meeting of this date, the following motion, of which notice had been given at last Meeting, was made by Dr CHRISTISON, seconded by Dr TRAILL, and unanimously carried,—“That Edinburgh having been fixed upon for the Twentieth Annual Meeting of the British Association for the Advancement of Science, which is to take place in August next, it is requisite that a Subscription be immediately set on foot for defraying the necessary expenses attendant on the reception of that Body.

“That the noblemen and gentlemen of Scotland generally, especially those resident in Edinburgh, be as soon as possible requested to enrol their names as subscribers to such a fund; and that a hope be expressed that Public Bodies and Chartered Societies will be induced to contribute their aid, as far as they may possess the means, in increasing the amount of this fund.

“That the Royal Society of Edinburgh having been one of the channels through which the invitation to the British Association to hold its Meeting this year in Edinburgh was conveyed, shall forthwith commence a Subscription among its own Fellows, and shall nominate a Committee to co-operate with the local Office-Bearers of the Association, and with the other Public Bodies in promoting this and other objects connected with the Meetings.

“That the sum of £50 be paid out of the funds of the Royal Society on account of this Subscription, and that all the Members of the Society be specially invited to contribute individually to this fund, it being understood that no donation should be of less amount than One Guinea.”

Monday, November 25, 1850.

At a Statutory General Meeting, Hon. Lord MURRAY, V.P., in the Chair, the following Office-Bearers were duly elected:—

Sir T. MAKDOUGALL BRISBANE, Bart., G.C.B., G.C.H., President.

Sir D. BREWSTER, K.H.,

Very Rev. Principal LEE,

Right Rev. Bishop TERROT,

Dr CHRISTISON,

Dr ALISON,

Hon. Lord MURRAY.

Professor FORBES, General Secretary.

Dr GREGORY,

Professor SMYTH,

Vice-Presidents.

Secretaries to the Ordinary Meetings.

JOHN RUSSELL, Esq., Treasurer.
 Dr TRAILL, Curator of Library and Instruments.
 JAMES WILSON, Esq., Curator of Museum.

COUNSELLORS.

JAMES DALMAHOY, Esq.	Dr J. Y. SIMPSON.
Dr GEORGE WILSON.	His Grace the Duke of ARGYLL.
Sir JOHN McNEILL, G.C.B.	Hon. Lord IVORY.
JOHN CAY, Esq.	Rev. Dr FLEMING.
Professor KELLAND.	Professor GOODSIR.
Very Rev. E. B. RAMSAY.	Rev. J. HANNAH.

The following Committee was appointed to audit the Treasurer's accounts :—

J. T. GIBSON-CRAIG, Esq. JOHN CAY, Esq. JAMES WALKER, Esq.

The Meeting then adjourned.

(Signed) JOHN A. MURRAY, V.P.

Memorandum.—January 20, 1851.—At the Ordinary Meeting of this date it was agreed, on the motion of His Grace the Duke of ARGYLL, seconded by Professor FORBES, that this Society should address Her Majesty's Government in favour of a more urgent prosecution of the Trigonometrical Survey of Scotland. A remit was made to the Council to prepare and transmit a Memorial on the subject, and the names of ALEXANDER KEITH JOHNSTON, Esq., and DAVID MILNE, Esq., were added for that purpose to those of the Council Committee.

Monday, November 24, 1851.

At a Statutory General Meeting, JOHN RUSSELL, Esq., Treasurer, in the Chair, the following Office-Bearers were duly elected :—

Sir T. MAKDOUGALL BRISBANE, Bart., G.C.B., G.C.H., President.	
Sir D. BREWSTER, K.H.,	} Vice-Presidents.
Very Rev. Principal LEE.	
Right Rev. Bishop TERROT.	
Dr CHRISTISON.	
Dr ALISON.	
Hon. Lord MURRAY.	
Professor FORBES, General Secretary.	
Dr GREGORY,	} Secretaries to the Ordinary Meetings.
Professor SMYTH,	
JOHN RUSSELL, Esq., Treasurer.	
Dr TRAILL, Curator of Library and Instruments.	
JAMES WILSON, Esq., Curator of Museum.	

COUNSELLORS.

Professor KELLAND.

Dr J. Y. SIMPSON.

His Grace the Duke of ARGYLL.

Hon. Lord IVORY.

Rev. Dr FLEMING.

Professor GOODSIR.

Rev. J. HANNAH.

Dr ANDERSON.

ROBERT CHAMBERS, Esq.

J. T. GIBSON-CRAIG, Esq.

Wm. SWAN, Esq.

Professor WILLIAM THOMSON.

The following Committee was appointed to audit the Treasurer's accounts :—

J. T. GIBSON-CRAIG, Esq., W.S.

JAMES WALKER, Esq., W.S.

WILLIAM T. THOMSON, Esq.

The Meeting then adjourned.

(Signed) JOHN RUSSELL.

Memorandum.—November 24, 1851.—At a Statutory General Meeting of this date, JOHN RUSSELL, Esq., Treasurer, in the Chair, a letter from Sir THOMAS M. BRISBANE, Bart., to the Secretary was read, stating that as, on account of the illness of one of his family, he intended passing the winter in England, he considered it right to tender his resignation as President of the Society.

It was moved by the Chairman, and unanimously agreed to, that "the Meeting should direct the Secretary to express to Sir THOMAS M. BRISBANE their regret at the cause which is likely to prevent his attendance this session; but, as they trust that this cause will be temporary, they would request him to withdraw his resignation, and hope that at a future period he will be able to resume the Chair, the duties of which he has so ably discharged."

Memorandum.—December 12, 1851.—At a Meeting of Council of this date, the acting General Secretary read extract of letter from Sir T. M. BRISBANE to Professor FORBES, in which he expressed his compliance with the request of the Society, made at the first General Meeting of this session, that he would withdraw his resignation of the Presidentship which he had then tendered.

Memorandum.—January 5, 1852.—At an Ordinary Meeting of this date, Sir D. BREWSTER, V.P., in the Chair, a Memorial to the Lords of the Treasury regarding the formation in Edinburgh of a Museum of Economic Geology, and praying for the extension of the Geological Survey to Scotland, was read and approved of, and ordered to be transmitted, after signature by Sir THOMAS M. BRISBANE, to the Treasury.

N.B.—The Memorial above referred to was transmitted accordingly, and its receipt acknowledged, on 13th February 1852, by Mr GEORGE CORNWALL LEWIS, the Secretary to the Treasury.

Monday, November 22, 1852.

At a Statutory General Meeting, Right Rev. Bishop TERROT, V.P., in the Chair, the following Office-Bearers were duly elected :—

Sir T. MAKDOUGALL BRISBANE, Bart., G.C.B., G.C.H., President.

Sir D. BREWSTER, K.H.,

Very Rev. Principal LEE,

Right Rev. Bishop TERROT,

Dr CHRISTISON,

Dr ALISON,

Hon. Lord MURRAY,

Professor FORBES, General Secretary.

Dr GREGORY,

Professor SMYTH,

JOHN RUSSELL, Esq., Treasurer.

Dr TRAILL, Curator of Library and Instruments.

JAMES WILSON, Esq., Curator of Museum.

Vice-Presidents.

Secretaries to the Ordinary Meetings.

COUNSELLORS.

Rev. Dr FLEMING.

Professor GOODSIR.

Rev. J. HANNAH.

Dr ANDERSON.

ROBERT CHAMBERS, Esq.

J. T. GIBSON-CRAIG, Esq.

WM. SWAN, Esq.

Professor WILLIAM THOMSON.

Dr J. H. BENNETT.

Dr J. H. BALFOUR.

ANDREW COVENTRY, Esq.

Rev. Dr JAMES GRANT.

The following Committee was appointed to audit the Treasurer's accounts:—

J. T. GIBSON-CRAIG, Esq.

JOHN MACKENZIE, Esq.

JAMES CUNNINGHAM, Esq. W.S.

The Meeting then adjourned.

(Signed)

C. H. TERROT, V.P.

Memorandum.—Monday, November 22, 1852.—At a Statutory General Meeting of this date, the Right Rev. Bishop TERROT in the Chair, a letter was read from Professor FORBES of 6th curt., in which, in consequence of the state of his health requiring a renewed leave of absence from Edinburgh, he unreservedly resigned the office of General Secretary, which the Society had done him the honour to confer upon him for a series of years, an honour which he stated he ever highly prized. After a few observations by the Right Rev. Chairman, expressive of the Society's regret at this intimation, and of the high estimation in which Professor FORBES was deservedly held (in which observations the Society most cordially concurred), it was moved by Professor CHRISTISON, "That Professor FORBES be requested to withdraw his resignation." This motion was immediately and unanimously adopted by the Society, and the acting General Secretary was instructed to communicate the resolution to Professor FORBES.

Memorandum.—In reference to Professor FORBES's proposed resignation of the General Secretaryship, and the resolution of the Society regarding it, as recorded in the preceding Minutes of the last Statutory General Meeting, it is to be here noted that the acting General Secretary communicated to Professor FORBES the unanimous resolution and request of the Society that he should withdraw his resignation, and that a letter of date the 24th day of November was duly received from Professor FORBES, in which, in accordance with the wishes of the Society, he withdrew his resignation.

LIST OF MEMBERS ELECTED.

MEMBERS ELECTED.

December 3, 1849.

His Grace the Duke of ARGYLL.

December 17, 1849.

The Most Noble the Marquis of TWEEDDALE.

January 7, 1850.

W. J. MACQUORN RANKINE, Esq., C.E.

January 21, 1850.

ALEX. KEITH JOHNSTON, Esq.

Dr JOHN SCOTT, F.R.C.P.

Dr SHERIDAN MUSPRATT, Liverpool.

February 18, 1850.

Dr JAMES STARK. (Re-admitted).

March 4, 1850.

Lieut. W. DRISCOLL GOSSET, R.E.

Dr WILLIAM SELLER, P.R.C.P.E.

March 18, 1850.

Professor BLACKBURN, Glasgow.

THOMAS GRAINGER, Esq., C.E.

April 15, 1850.

ALEXANDER KEMP, Esq.

December 2, 1850.

Dr R. D. THOMSON, Glasgow.

Dr MORTIMER GLOVER, Newcastle.

December 16, 1850.

BERIAH BOTFIELD, Esq.

Dr J. S. COMBE.

Dr SPITTAL. (Re-admitted).

February 3, 1851.

Sir DAVID DUNDAS, Bart.

February 17, 1851.

Sir GEORGE DOUGLAS, Bart.

March 3, 1851.

JOHN STEWART, Esq.

Dr JOHN KINNIS.

April 7, 1851.

E. W. DALLAS, Esq.

LIST OF MEMBERS ELECTED.

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April 21, 1851.

Rev. Dr JAMES GRANT.

December 15, 1851.

Rev. A. BARRY, Glenalmond.

Sir JAMES RAMSAY, Bart.

January 19, 1852.

ETRE B. POWELL, Esq., Madras.

THOMAS MILLER, Esq., Perth Academy.

ALLEN DALZELL, Esq.

February 2, 1852.

Dr JOHN WYLIE, late Physician-General, Madras.

February 16, 1852.

JAMES CUNNINGHAM, Esq., W.S.

April 5, 1852.

JAMES W. GRANT, Esq. of Elchies.

December 6, 1852.

ALEX. JAMES RUSSELL, Esq., C.S.

Dr ANDREW FLEMING, Bengal.

January 4, 1853.

Major EDWARD MADDEN.

Dr JAMES WATSON, Bath.

Lieut. ROBERT MACLAGAN, Bengal Engineers.

February 7, 1853.

Rev. Dr ROBERT LEE.

Professor J. S. BLACKIE.

Right Rev. Bishop TROWER, D.D.

February 21, 1853.

JAMES M. HOG, Esq. of Newliston.

Rev. JOHN CUMMING, D.D., London.

LIST OF THE PRESENT ORDINARY MEMBERS,

IN THE ORDER OF THEIR ELECTION.

General Sir THOMAS M. BRISBANE, Bart., G.C.B. &c., F.R.S. Lond.

PRESIDENT.

Date of
Election.

- 1798 Alexander Monro, M.D.
1799 Robert Jameson, Esq., *Professor of Natural History.*
1807 John Campbell, Esq., *of Carbrook.*
1808 James Wardrop, Esq.
Sir David Brewster, K.H., LL.D. F.R.S, Lond., *St Andrews.*
1811 General Sir Thomas Makdougall Brisbane, Bart., G.C.B., G.C.H., F.R.S. Lond.
James Jardine, Esq., *Civil Engineer.*
J. G. Children, Esq., F.R.S. Lond.
Alexander Gillespie, Esq., *Surgeon.*
W. A. Cadell, Esq., F.R.S. Lond.
1812 James Pillans, Esq., *Professor of Humanity.*
Sir George Clerk, Bart., F.R.S. Lond.
1813 William Somerville, M.D., F.R.S. Lond.
1814 Right Honourable Lord Viscount Arbutnot.
John Fleming, D.D., *Professor of Natural Science, New College.*
Alexander Brunton, D.D.
1815 Henry Home Drummond, Esq., *of Blair-Drummond.*
William Thomas Brande, Esq., F.R.S. Lond., *Professor of Chemistry in the Royal Institution.*
1816 Leonard Horner, Esq., F.R.S. Lond.
Honourable Lord Fullerton.
1817 John Wilson, Esq., *late Professor of Moral Philosophy.*
Alexander Maconochie, Esq., *of Meadowbank.*
William P. Alison, M.D., *Professor of the Practice of Physic.*
Robert Bald, Esq., *Civil Engineer.*

LIST OF ORDINARY MEMBERS.

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Date of
Election.

- 1818 Robert Richardson, M.D., *Harrogate*.
Patrick Miller, M.D., *Exeter*.
John Watson, M.D.
Right Honourable John Hope, *Lord Justice-Clerk*.
- 1819 Patrick Murray, Esq., *of Simprim*.
Thomas Stewart Traill, M.D., *Professor of Medical Jurisprudence*.
Alexander Adie, Esq.
Marshall Hall, M.D., *London*.
Richard Phillips, Esq., F.R.S. *Lond*.
Reverend William Scoresby, *Exeter*.
George Forbes, Esq.
- 1820 James Keith, M.D., *Surgeon*.
Charles Babbage, Esq., F.R.S. *Lond*.
Sir John F. W. Herschel, Bart., F.R.S. *Lond*.
John Shank More, Esq., *Professor of Scots Law*.
Robert Haldane, D.D., *Principal of St Mary's College, St Andrews*.
Dr William Macdonald, *Professor of Natural History, St Andrews*.
Sir John Hall, Bart., *of Dunglass*.
Sir George Ballingall, M.D., *Professor of Military Surgery*.
- 1821 Sir James M. Riddell, Bart., *of Ardnamurchan*.
Archibald Bell, Esq., *Advocate*.
John Clerk Maxwell, Esq., *Advocate*.
John Lizars, Esq., *Surgeon*.
John Cay, Esq., *Advocate*.
Robert Kaye Greville, LL.D.
Robert Hamilton, M.D.
- 1822 James Smith, Esq., *of Jordunhill*, F.R.S. *Lond*.
William Bonar, Esq.
George A. Walker-Arnott, LL.D., *Professor of Botany, Glasgow*.
Very Reverend John Lee, D.D., *Principal of the University of Edinburgh*.
Sir James South, F.R.S. *Lond*.
Lieutenant-General Martin White.
Walter Frederick Campbell, Esq.
Sir W. T. Trevelyan, Bart., *Nettlecombe, Somersetshire*.
Sir Robert Abercromby, Bart., *of Birkenbog*.
Dr Wallich, *Calcutta*.
John Russell, Esq., P.C.S.
John Dewar, Esq., *Advocate*.
- 823 Sir Edward Ffrench Bromhead, Bart., A.M., F.R.S. *Lond*, *Thursby Hall*.
Captain Thomas David Stuart, *of the Hon. East India Company's Service*.
Andrew Fyfe, M.D., *Professor of Medicine and Chemistry, King's College, Aberdeen*.
Robert Bell, Esq., *Advocate*.
Admiral Norwich Duff.
Warren Hastings Anderson, Esq.

Date of
Election.

- 1823 Alexander Thomson, Esq., of *Banchory*.
 Liscombe John Curtis, Esq., *Ingsdon House, Devonshire*.
 Robert Christison, M.D., *Professor of Materia Medica*.
 John Gordon, Esq., of *Cairnbulg*.
- 1824 Alexander Wilson Philip, M.D., *London*.
 Robert E. Grant, M.D., *Professor of Comparative Anatomy, University College, London*.
 Reverend Dr William Muir, *one of the Ministers of Edinburgh*.
 W. H. Playfair, Esq., *Architect*.
 John Argyle Robertson, Esq., *Surgeon*.
 James Pillans, Esq.
 James Walker, Esq., *Civil Engineer*.
 William Wood, Esq., *Surgeon*.
- 1825 Honourable Lord Wood.
- 1826 Sir David Hunter Blair, Bart.
 Dr John Macwhirter.
- 1827 John Gardiner Kinnear, Esq.
 James Russell, M.D.
 Reverend Dr Robert Gordon, *one of the Ministers of Edinburgh*.
 James Wilson, Esq.
 Very Reverend Edward Bannerman Ramsay, A.M., *Camb.*
 George Swinton, Esq.
- 1828 Erskine Douglas Sandford, Esq., *Advocate*.
 David Maclagan, M.D.
 Sir William A. Maxwell, of *Calderwood, Bart.*
 John Forster, Esq., *Architect, Liverpool*.
 Thomas Graham, A.M., *Professor of Chemistry, London University*.
 David Milne Home, Esq., *Advocate*.
 Dr Manson, *Nottingham*.
 William Burn Callander, Esq., of *Prestonhall*.
- 1829 A. Colyar, Esq.
 Sir William Gibson-Craig, Bart.
 James Ewing, LL.D., *Glasgow*.
 Right Honourable Duncan M'Neill, *Lord Justice-General*.
 Venerable Archdeacon Sinclair, *Kensington*.
 Arthur Connell, Esq., *Profesor of Chemistry, St Andrews*.
 Bindon Blood, Esq., M.R.I.A.
 James Walker, Esq., W.S.
 William Bald, Esq., M.R.I.A.
- 1830 J. T. Gibson-Craig, Esq., W.S.
 Sir Archibald Alison, Bart., *Sheriff of Lanarkshire*.
 Honourable Mountstuart Elphinstone.
 James Syme, Esq., *Professor of Clinical Surgery*.
 James L'Amy, Esq., *Sheriff of Forfarshire*.
 Thomas Barnes, M.D., *Carlisle*.

Date of
Election.

- 1831 James D. Forbes, Esq., F.R.S. Lond., *Professor of Natural Philosophy.*
 Right Honourable Lord Dunfermline.
 Donald Smith, Esq.
 O. Tyndal Bruce, Esq., of *Falkland.*
 David Boswell Reid, M.D., *London.*
- 1832 John Sligo, Esq., of *Carmyle.*
 James F. W. Johnston, A.M., *Professor of Chemistry in the University of Durham.*
 William Gregory, M.D., *Professor of Chemistry.*
 Robert Allan, Esq., *Advocate.*
 Robert Morrieson, Esq., *Hon. E.I.C. Civil Service.*
 Montgomery Robertson, M.D.
- 1833 Captain Milne, R.N.
 His Grace the Duke of Buccleuch, K.G.
 David Craigie, M.D.
 Sir John Stuart Forbes, Bart., of *Pitsligo.*
 Alexander Hamilton, Esq., LL.B., W.S.
 Right Honourable Earl Cathcart.
- 1834 Mungo Ponton, Esq., W.S.
 Isaac Wilson, M.D., F.R.S. Lond.
 David Low, Esq., *Professor of Agriculture.*
 Patrick Boyle Mure Macredie, Esq., *Advocate.*
 John Davies Morries Stirling, Esq.
 Thomas Jameson Torrie, Esq.
 John Haldane, Esq., *Haddington.*
 William Sharpey, M.D., *Professor of Anatomy, University College, London.*
- 1835 John Hutton Balfour, M.D., *Professor of Botany.*
 Right Honourable Lord Campbell.
 William Brown, Esq., F.R.C.S.
 R. Mayne, Esq.
- 1836 David Rhind, Esq., *Architect.*
 Archibald Robertson, M.D., F.R.S. Lond.
- 1837 John Archibald Campbell, Esq., W.S.
 John Scott Russell, Esq., A.M.
 Charles Maclaren, Esq.
 Archibald Smith, Esq., M.A. Camb., *Lincoln's Inn, London.*
 Richard Parnell, M.D.
 Peter D. Handyside, M.D., F.R.C.S.
- 1838 Thomas Mansfield, Esq., *Accountant.*
 Alan Stevenson, Esq., *Civil Engineer.*
- 1839 David Smith, Esq., W.S.
 Adam Hunter, M.D.
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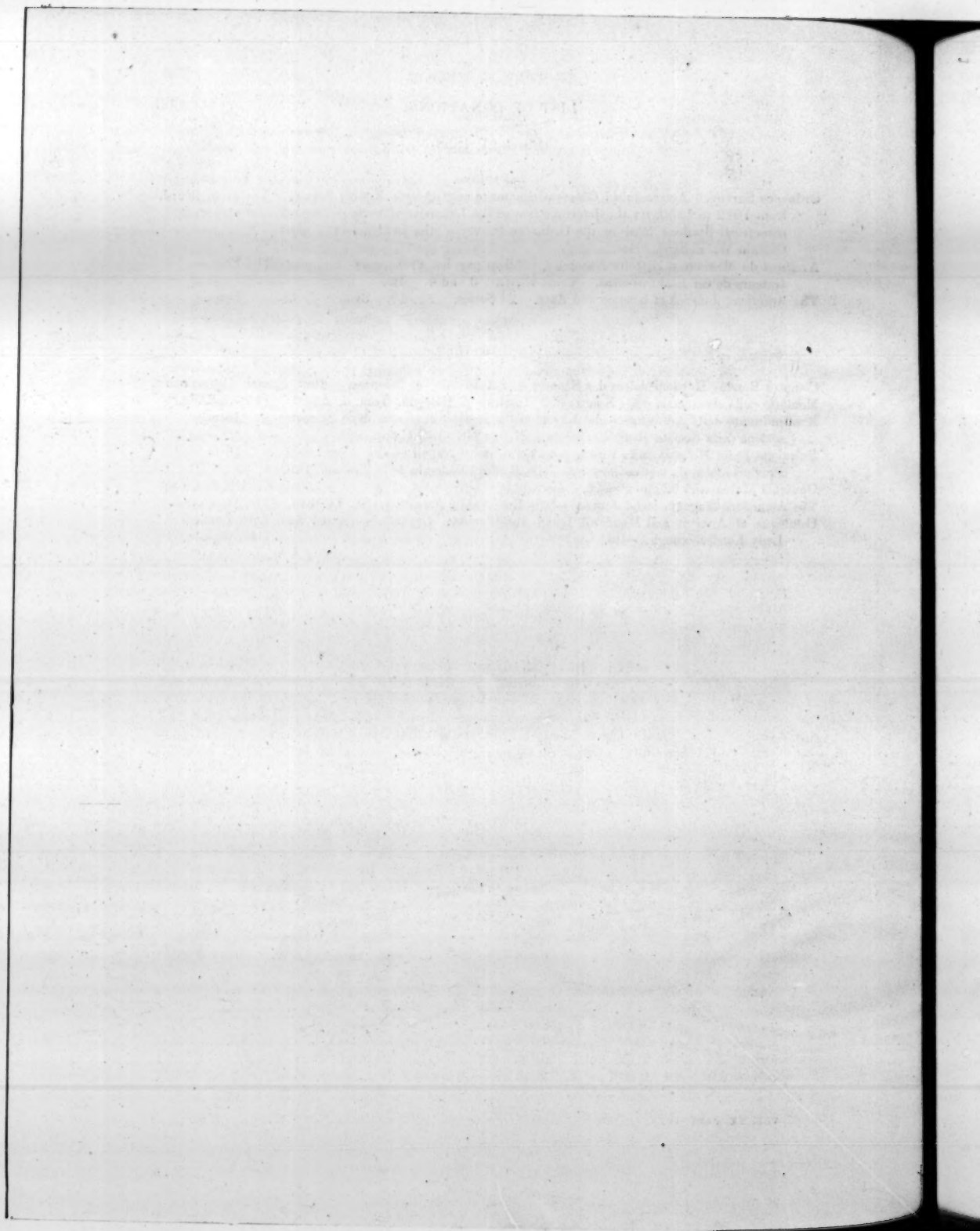
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10 DE 53

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[microform]. -- Vol. 1 ([1783/86])-v. 46 ([1907/09]). --
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Royal Society : Sold in London by T. Cadell, 1788-1910.
46 v. : ill.

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Some vols. have also a special title: Vols. 17-19, 22
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Indexes: Vols. 1 (1783)-13 (1836) in v. 13; v. 1 (1783)-

34 (1888) published separately (1 v.) in 1890; v. 35

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Edinburgh : Published by Robert Grant & Son ... and
Williams & Norgate ... London, 1857.

[8], ix, [3], 7, [1], 704 [i.e. 706], ii p., [16] leaves
of plates (1 folded)

Errors in pagination: p. 541-542 repeated in numbering.